SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE PHYSIOLOGICAL IMPORTANCE OF PHASE BOUNDARIES[1]

EVEN a hasty consideration of the arrangements present in living cells is sufficient to bring conviction that the physical and chemical systems concerned operate under conditions very different from those of reactions taking place between substances in true solution. We become aware of the fact that there are numerous constituents of the cell which do not mix with one another. In other words, the cell system is one of many "phases," to use the expression introduced by Willard Gibbs.

Further, parts of this system which appear homogeneous under the ordinary microscope are shown by the ultra-microscope to be themselves heterogeneous. These are in what is known as the colloidal state. Some dispute has taken place as to whether this state is properly to be called a heterogeneous one, but it is sufficient for our purpose to note that investigation shows that the interfaces of contact between the components of such systems are the seat of the various forms of energy which we meet with in the case of systems obviously consisting of phases which can be separated mechanically, so that considerations applying to coarsely heterogeneous systems apply also to colloidal systems. Although the phases of a colloidal system can not be so obviously and easily separated as those of an ordinary heterogeneous one, this can be done almost completely by filtration through membranes such as the gelatin in

[1] Address of the President of the Section of Physiology at the Manchester Meeting of the British Association for the Advancement of Science.

Martin's process. To avoid confusion, however, it has been suggested that the colloidal state should be spoken of as "microheterogeneous." There are, in fact, certain phenomena more or less peculiar to the colloidal state and due to the influence of the sharp curvature of the surfaces of the minutely subdivided phase. The effect of this curvature is a considerable pressure in the interior of the phase, owing to the surface tension, and it adds further complexity to the properties manifested by it.

We see, then, that the chemical reactions of chief importance to us as physiologists are those known as "heterogeneous." This class of reactions, until comparatively recent times, has been somewhat neglected by the pure chemist.

In some of its aspects, the problem before us was discussed by one of my predecessors, Professor Hopkins, as also by Professor Macallum, but its importance will, I think, warrant my asking your indulgence for a further brief discussion. Permit me first to apologize for what may seem to some of those present to be an unnecessarily elementary treatment of certain points.

It is easy to realize that the molecules which are situated at the interface where two phases are in contact are subject to forces differing from those to which the molecules in the interior of either phase are subject. Consider one phase only, the molecules at its surface are exposed on the one side to the influence of similar molecules; on the other side, they are exposed to the influence of molecules of a nature chemically unlike their own or in a different physical state of aggregation. The result of such asymmetric forces is that the phase boundary is the seat of various forms of energy not present in the interior of the phase. The most obvious of these is the surface energy due to the state of tension existing where a liquid or a gas forms one of the phases. It would lead us too far to discuss the mode of origin of this surface tension, except to call to mind that it is due to the attractive force of the molecules for one another, a force which is left partially unbalanced at the surface, so that the molecules here are pulled inwards. The tension is, of course, only the intensity factor of the surface energy, the capacity factor being the area of the surface. We see at once that any influence which alters the area of the surface alters also the magnitude of that form of energy of which we are speaking.

This is not the only way in which the properties of substances are changed at phase boundaries. The compressibility of a solvent, such as water, are altered, so that the solubilities of various substances in it are not the same as in the interior of the liquid phase. It is stated by J. J. Thomson that potassium sulphate is 60 per cent. more soluble in the surface film. The ways in which the properties of a solvent are changed are sometimes spoken of as "lyotropic," and they play an important part in the behavior of colloids. We meet also with the presence of electrical charges, of positive or negative sign. These are due, as a rule, to electrolytic dissociation of the surface of one phase, in which the one ion, owing to its insolubility, remains fixed at the surface, while the opposite ion, although soluble, can not wander away further than permitted by electrostatic attraction. Thus we have a Helmholtz double layer produced.

Before we pass on to consider how these phenomena intervene in physiological processes, there is one fact that should be referred to on account of its significance in connection with the contractile force of muscle. Surface tension is found to *decrease* as the temperature rises, or, as it is sometimes put, it has a negative tempera-

ture coefficient. This is unusual; but, if we remember that the interface between a liquid and its vapor disappears when the temperature rises to the critical point, and with it, of course, all phenomena at the boundary surface, the fact is not surprising that there is a diminution of these phenomena as the critical temperature is approached.

Perhaps that result of surface energy known as "adsorption" is the one in which the conditions present at phase boundaries make themselves most frequently obvious. Since the name has been used somewhat loosely, it is a matter of some consequence to have clear ideas of what is meant when it is made use of. Unless it is used to describe a definite fact, it can only be mischievous to the progress of science.

Permit me, then, first to remind you of that fact of universal experience, known as the "dissipation of energy," which is involved in the second law of energetics. Free energy—that is, energy which can be used for the performance of useful work—is invariably found to diminish, if the conditions are such that this is possible. If we have, therefore, a system in which, by any change of distribution of the constituents, free energy can be decreased, such a change of distribution will take place. This is one form of the well-known "Principle of Carnot and Clausius."

Now, practically any substance dissolved in water lowers the surface tension present at the interface between the liquid and another solid or liquid phase with which it is in contact. Moreover, up to a certain limit, the magnitude of this effect is in proportion to the concentration of the solute. Therefore, as was first pointed out by Willard Gibbs, concentration of a solute at an interface has the effect of reducing free energy and will therefore occur. This is adsorption. As an example, we may take the deposition of a dyestuff on the surface of charcoal, from which it can be removed again, unaltered, by appropriate means, such as extraction with alcohol. Charcoal plus dye may, if any satisfaction is derived from the statement, be called a compound. But, since its chemical composition depends on the concentration of the solution in which it was formed, it is much more accurate to qualify the statement by calling it an "adsorption-compound." Moreover, the suggestion that the union is a chemical one tends to deprive the conception of chemical combination of its characteristic quality, namely, the change of properties. Dye-stuff and charcoal are chemically unchanged by adsorption.

The origin of adsorption from surface tension is easily able to explain why it is less as the temperature rises, as we find experimentally. As we have just seen, surface tension diminishes with increase of temperature.

Let us next consider what will happen if the liquid phase contains in solution a substance which lowers surface tension and is also capable of entering into chemical reaction with the material of which the other, solid, phase consists. For example, a solution of caproic acid in contact with particles of aluminium hydroxide. On the surface of the solid, the concentration of the acid will be increased by adsorption, and, in consequence, the rate of the reaction with it will be raised, according to the law of mass action. Further, suppose that the liquid phase contains two substances which react slowly with each other, but not with the solid phase. They will be brought into intimate contact with each other on the surface of the solid phase, their concentration raised and the rate of their interaction increased. One of the reagents may clearly be the solvent itself. But in all these cases the rate of the reaction can not

be expressed by a simple application of the law of mass action, since the active masses are not functions of the molecular concentrations, but of the surface of the phase boundaries. The application of these considerations to the problem of the action of enzymes and of heterogeneous catalysis in general will be apparent. That the action of enzymes is exerted by their surfaces is shown, apart from the fact that they are in colloidal solution, by the results of experiments made in liquids in which the enzymes themselves are insoluble in the usual sense, so that they can be filtered off by ordinary filter paper and the filtrate found to be free from enzyme. Notwithstanding this insolubility, enzymes are still active in these liquids. The statement has been found, up to the present, to apply to lipase, emulsin and urease, probably to trypsin, and the only difficulty in extending it to all enzymes is that of finding a substrate soluble in some liquid in which the enzyme itself is not. That adsorption is a controlling factor in the velocity of enzyme action has been advocated by myself for some years, but it is not to be understood as implying that the whole action of enzymes is an "adsorption phenomenon," whatever may be the meaning of this statement. The rate at which the chemical reaction proceeds is controlled by the mass of the reagents concentrated on the surface of the enzyme phase at any given moment, but the temperature coefficient will, of course, be that of a chemical reaction.

The thought naturally suggests itself, may not the adsorption of the reacting substances on the surface of the enzyme suffice in itself to bring about the equilibrium at a greater rate, so that the assumption of a secondary chemical combination of a chemical nature between enzyme and substrate may be superfluous? I should hesitate somewhat to propose this hypothesis for serious consideration were it not that it was given by Faraday as the explanation of one of the most familiar cases of heterogeneous catalysis, namely, the union of oxygen and hydrogen gases by means of the surfaces of platinum and other substances. The insight shown by Faraday into the nature of the phenomena with which he was concerned is well known and has often caused astonishment. Now, this case of oxygen and hydrogen gases is clearly one of those called "catalytic" by Berzelius. The fact that the agent responsible for the effect did not itself suffer change was clear to Faraday. I would also, in parenthesis, direct attention to the fact that he correctly recognized the gold solutions which he prepared as suspensions of metallic particles —that is, as what we now call colloidal solutions. Although the systematic investigation of colloids, and the name itself, were due to Graham, some of the credit of the discovery should be given to the man who first saw what was their nature. Adsorption, again, was accurately described by Faraday, but without giving it a name.

I confess that there are, at present, difficulties in the way of accepting concentration by adsorption as a complete explanation of the catalytic activities of enzymes. It is not obvious, for example, why the same enzyme should not be able to hydrolyze both maltose and saccharose, as it is usually expressed. Another difficulty is that it is necessary to assume that the relative concentration of the components of the chemical system must be the same on the surface of the enzyme as it is in the body of the solution; in other words, the adsorption of each must be the same function of its concentration. Unless this were so, the equilibrium position on the enzyme surfaces, and therefore in the body of the solution, would be a different one under the action of an enzyme from that arrived at spontaneously

or brought about by a homogeneous catalyst such as acid. This consideration was brought to my notice by Professor Hopkins, and requires experimental investigation. We know, indeed, that in some cases there is such a difference in the position of the equilibrium position, for which various explanations have been suggested. But it would be a matter of some interest to know whether this difference has any relation to different degrees of adsorption of the components of the system.

At the same time, adsorption is under the control of so many factors, surface tension, electrical charge, and so on, that the possibilities seem innumerable. There are, moreover, two considerations to which I may be allowed to direct your attention. Hardy has pointed out that it is probable that the increased rate of reaction at the interface between phases may be due, not merely to increased concentration as such, but that in the act of concentration itself molecular forces may be brought into play which result in a rise in chemical potential of the reacting substances. In the second place, Barger has shown that the adsorption of iodine by certain organic compounds is clearly related to the chemical composition of the surfaces of these substances, but that this relationship does not result in chemical combination or in abolition of the essential nature of the process as an adsorption. It would appear that those properties of the surface, such as electric charge and so on, which control the degree of adsorption, are dependent on the chemical nature of the surface. This dependence need not cause us any surprise, since the physical properties of a substance, inclusive of surface tension, are so closely related to its chemical composition.

There is one practical conclusion to be derived from the facts already known with regard to enzymes. This is, that any simple application of the law of mass action can not lead to a correct mathematical expression for the rate of reaction, although attempts of this kind have been made, as by Van Slyke. The rate must be proportional to the amount of substrate adsorbed, and this, again, is a function both of the concentration of the substrate and of that of the products. It is, then, a continuously varying quantity. Expressed mathematically, the differential equation for the velocity must be something of this kind:

$$\frac{dC}{dt} = KC^n$$

where n itself is an unknown function of C, the concentration of the substrate or products.

The hypothesis of control by adsorption gives a simple explanation of the exponential ratio between the concentration of the enzyme and its activity, which is found to be different numerically according to the stage of the reaction. At the beginning, it may be nearly unity; in the middle it is more nearly 0.5, as in the so-called "square root law" of Schütz and Borissov, which is, however, merely an approximation. Simple explanations are also given of the fact that increasing the concentration of the substrate above a certain value no longer causes an increased rate of reaction. This is clearly because the active surface is saturated. Again, the effect of antiseptics and other substances which, by their great surface activity, obtain possession of the enzyme surfaces, and thereby exclude to a greater or less degree the adsorption of the substrate, receives a reasonable account. In many cases, the depressant or favoring action of electrolytes, including acid and alkali, is probably due to aggregation or dispersion of the colloidal particles of the enzyme, with decrease or increase of their total surface. It is to be noted that such

explanations are independent of any possible formation of an intermediate compound between enzyme and substrate, *after* adsorption has taken place.

There is a further way in which adsorption plays a part in the chemical processes of cells, including those under the influence of catalysts. It is a familiar fact that the concentration of water plays a large part in the position of equilibrium attained in reversible reactions of hydrolysis and synthesis. A synthetic process is brought about by diminution of the effective concentration of water. There are, doubtless, means of doing this in the elaborate mechanisms of cell life, and, in all probability, it is by adsorption on surfaces, which are able to change their "affinity" for water.

I pass on to consider briefly some other cases in which the phenomena at phase boundaries require attention.

Let us turn our gaze from the interior of the cell to the outer surface, at which it is in contact with the surrounding medium. From the nature of adsorption there can be no doubt that, if the cell or the surrounding liquid contains substances which decrease surface energy of any form, these constituents will be concentrated at the interface. There are many such substances to be found in cells, some of lipoid nature, some proteins, and so on. Further, the experiments of Ramsden have shown that a large number of substances are deposited in surface films in a more or less rigid or solidified form. We are thus led to inquire whether these phenomena do not account for the existence of the cell membrane, about which so much discussion has taken place. We find experimentally that there are facts which show that this membrane, under ordinary resting conditions, is impermeable to most crystalloids, including inorganic salts, acids and bases. There is no other explanation of the fact that the salts present in cells are not only in different concentration inside from that outside, but that there may be absence of certain salts from one which are present in the other, as, for example, sodium in the plasma of the rabbit not in the corpuscles. Moreover, the experiments of Hoeber have shown that electrolytes are free in the cells, so that they are not prevented from diffusion by being fixed in any way. The mere assumption of a membrane impermeable to colloids only will not account for the facts, since, as I have shown in another place, this would only explain differences of concentration, but not of composition. The surface concentration of cell constituents readily accounts for the changes of permeability occurring in functional activity, since it depends on the nature of the cell protoplasm, and chemical changes of many and various kinds occur in this system. If such be the nature of the cell membrane, it is evident that we are not justified in expecting to find it composed of lipoid or of protein alone. It must have a very complex composition, varying with the physiological state of the cell. Indeed, complex artificial membranes have been prepared having properties very similar to that of the cell.

This view that the membrane is formed by surface condensation of constituents of the cell readily accounts for the changes of permeability occurring in functional activity, since its composition depends on that of the cell protoplasm, and chemical changes of various kinds take place in this system, as it is scarcely necessary to remind you. In fact, the cell membrane is not to be regarded as an independent entity, but as a working partner, as it were, in the business of the life of the cell. In the state of excitation, for example, there is satisfactory evidence that the cell membrane loses its character of semipermeability to electrolytes, etc. This statement has been shown

to apply to muscle, nerve, gland cells, and the excitable tissues of plants, as well as to unicellular organisms. We shall see presently how this fact gives a simple explanation of the electrical changes associated with the state of activity.

If, then, the cell membrane is a part of the cell system as a whole, it is not surprising to find that substances can affect profoundly, although reversibly, the activities of the cell, even when they are unable to pass beyond the outer surface. The state of dynamic equilibrium between the cell membrane and the rest of the cell system is naturally affected by such means, since the changes in the one component involve compensating ones in the other. Interesting examples of such actions are numerous. I may mention the effect of calcium ions on the heart muscle, the effect of sodium hydroxide on oxidation in the eggs of the sea-urchin, and that of acids on the contraction of the jelly-fish. Somewhat puzzling are those cases in which drugs, such as pilocarpine and muscarine, act only during their passage through the membrane and lose their effect when their concentration has become equal inside and outside the cell.

The work of Dale on anaphylaxis leads him to the conclusion that the phenomena shown by sensitized plain muscle can most reasonably be explained by colloidal interaction on the surface of the fibers. The result of this is increased permeability and excitation resulting therefrom.

I referred previously to the electrical change in excitable tissues and its relation to the cell membrane. It was, I believe, first pointed out by Ostwald and confirmed by many subsequent investigators, that in order that a membrane may be impermeable to a salt it is not a necessary condition that it shall be impermeable to both the ions into which this salt is electrolytically dissociated. If impermeable to one only of these ions, the other, diffusible, ion can not pass out beyond the point at which the osmotic pressure due to its kinetic energy balances the electrostatic attraction of the oppositely charged ion, which is imprisoned. There is a Helmholtz double layer formed at the membrane, the outside having a charge of the sign of the diffusible ions, the inside that of the other ions. Now, suppose that we lead off from two places on the surface of a cell having a membrane with such properties to some instrument capable of detecting differences of electrical potential. It will be clear that we shall obtain no indication of the presence of the electrical charge, because the two points are equipotential, and we can not get at the interior of the cell without destroying its structure. But if excitation means increased permeability, the double layer will disappear at an excited spot owing to indiscriminate mixing of both kinds of ions, and we are then practically leading off from the interior of the cell, that is, from the internal component of the double layer, while the unexcited spot is still led off from the outer component. The two contacts are no longer equipotential. Since we find experimentally that a point at rest is electrically positive to an excited one, the outer component must be positive, or the membrane is permeable to certain cations, impermeable to the corresponding anions. Any action on the cell such as would make the membrane permeable, injury, certain chemical agents, and so on, would have the same effect as the state of excitation. If we may assume the possibility of degrees of permeability, the state of inhibition might be produced by *decrease* of permeability of the membrane of a cell, which was previously in a state of excitation owing to some influence inherent in the cell itself or coming from the outside. This manner of account-

ing for the electromotive changes in cells is practically the same as that given by Bernstein.

It will be found of interest to apply to secretory cells the facts to which I have directed your attention. If we suppose that the setting into play of such cells is associated with the production of some osmotically active substance, together with abolition of the state of semi-permeability of the membrane covering the ends of the cells in relation with the lumen of the alveolus of the gland, it is plain that water would be taken up from the lymph spaces and capillaries and escape to the duct, carrying with it the secretory products of the cells. This process would be continuous so long as osmotically active substances were formed. Such a process has been shown by Lepeshkin to occur in plants, and we have also evidence of increased permeability during secretory activity in the gland cells of animals. From what has been said previously, it is evident that electrical differences would show themselves between the permeable and semipermeable ends of such cells, as has been found to be the case.

As a modifiable structure, we see the importance of such a membrane as that described if it takes part in the formation of the synapse between neurones. The manifold possibilities of allowing passage to states of excitation or inhibition and of being affected by drugs will be obvious without further elaboration on my part.

Enough has already been said, I think, to show the innumerable ways in which phenomena at phase boundaries intervene in physiological events. Indeed, there are very few of these, if any, in which some component or other is not controlled by the action of surfaces of contact. But there is one especially important case to which I may be allowed to devote a few words in conclusion. I refer to the contractile process of muscle. It has become clear, chiefly through the work of Fletcher, Hopkins and A. V. Hill, that what is usually called muscular contraction consists of two parts. Starting from the resting muscle, we find that it must have a store of potential energy, since we can make it do work when stimulated. After being used in this way, the store must be replenished, since energy can not be obtained from nothing. This restoration process is effected by an independent oxidation reaction, in which carbohydrate is burnt up with the setting free of energy which is made use of to restore the muscle to its original state. Confining our attention for the moment to the initial, contractile, stage, the essential fact is the production of a certain amount of energy of tension, which can either be used for the performance of external work or be allowed to become degraded to heat in the muscle itself. It was Blix who first propounded the view that the amount of this energy of tension is related to the magnitude of certain surfaces in the muscle fibers. But the fact was demonstrated in a systematic and quantitative manner by A. V. Hill. He showed, in fact, that the amount of energy set free in the contractile process is directly related to the length of muscle fibers during the development of the state of tension. In other words, the process is a surface phenomenon, not one of volume, and is directly proportional to the area of certain surfaces arranged longitudinally in the muscle. This same relationship has been shown by Patterson and Starling to hold for the ventricular contraction of the mammalian heart and by Kosawa for that of the cold-blooded vertebrate. It appears that all the phenomena connected with the output of blood by the heart can be satisfactorily explained by the hypothesis that the energy of the contraction is regulated by the *length* of the ventricular fibers during the period

of development of the contractile stress. The degree of filling at the moment of contraction is thus the determining factor.

That surface tension itself may be responsible for the energy given off in muscular contraction was first suggested by Fitzgerald in 1878, and it seems, from calculations made, that changes at the contact surface of the fibrillæ with the sarcoplasm may be capable of affording a sufficient amount. The difficulties in deciding the question are great, but, in addition to the facts mentioned, there is other interesting evidence at hand. It has been shown, by Gad and Heymans, by Bernstein and others, that the contractile stress produced by a stimulus has a negative temperature coefficient. Within the limits of temperature between which the muscle can be regarded as normal, this stress is the greater the lower the temperature. The same statement was shown by Weizsäcker (working with A. V. Hill) to hold for the heat developed in the contractile stage. Now, of all the forms of energy possibly concerned, that associated with phase boundaries is the only one with a negative temperature coefficient. Another aspect of this relation to temperature is the well-known increase of the tonus of smooth muscle with fall in temperature.

It is tempting to bring into relation with the change in surface tension the production of lactic acid. In fact, this idea was put into a definite statement by Haber and Klemensievich in 1909 in a frequently quoted paper on the forces present at phase boundaries. The production of acid is stated to alter the electrical forces at this situation. This electrical charge involves a change of surface tension, and it is this change of surface tension which brings about the mechanical deformation of the muscle. Mines also has brought forward good evidence that the production of lactic acid is responsible for the change of tension. As to how the lactic acid is set free, and of what nature the system of high potential present in muscle may be, we require much more information. The absence of evolution of carbon dioxide when oxygen is not present shows that no oxidation takes place in the development of tension. There are other difficulties also in supposing that this system present in resting muscle is of a chemical nature. If the energy afforded by the oxidation of carbohydrate in the recovery stage is utilized for the formation of another chemical system with high energy content, the theory of coupled reactions indicates that there must be some component common to both systems. It is difficult to see what component of the muscle system could satisfy the conditions required. On the whole, some kind of system of a more physical nature seems the most probable. If it be correct that the oxidation of substances other than carbohydrate, fat, for example, can afford the chemical energy for muscular contraction, as appears from the results of metabolism experiments, a further difficulty arises in respect to a coupled reaction. But the question still awaits investigation.

On the whole, I think that we may conclude that more study of the phenomena at phase boundaries will throw light on many problems still obscure. It would probably not be going too far to say that the peculiarities of the phenomena called "vital" are due to the fact that they are manifestations of interchange of energy between the phases of heterogeneous systems. It was Clerk Maxwell who compared the transactions of the material universe to mercantile operations in which so much credit is transferred from one place to another, energy being the representative of credit. There are many indications that it is just in this process of change of energy from one form

to another that special degrees of activity are to be observed. Such, for example, are the electrical phenomena seen in the oxidation of phosphorus or benzaldehyde, and it appears that, in the photo-chemical system of the green plant, radiant energy is caught on the way, as it were, to its degradation to heat, and utilized for chemical work. In a somewhat similar way, it might be said that money in the process of transfer is more readily diverted, although perhaps not always to such good purpose as in the chloroplast. Again, just as in commerce money that is unemployed is of no value, so it is in physiology. Life is incessant change or transfer of energy, and a system in statical equilibrium is dead.

W. M. Bayliss

University College,
London

THE PUBLICATION OF THE RESULTS OF INVESTIGATIONS MADE IN EXPERIMENT STATIONS IN TECHNICAL SCIENTIFIC JOURNALS[1]

In order to gain a proper perspective for a consideration of the topic which has been assigned the final place in this discussion of experiment station publications, namely, the publication of results in scientific journals, including the *Journal of Agricultural Research*, it will be necessary to consider very briefly certain historical aspects of the question. Until within the last few years it has been a well-nigh universal practise of the experiment stations in this country to publish all, or very nearly all, of the material which they have had for publication in the form of bulletins. The reason for this practise, which has always seemed anomalous to scientific workers in other than agricultural fields, is of course found in the historical beginnings of station work and station publication in America.

Section 1 of the Hatch Act provides "That in order to aid in acquiring and diffusing among the people of the United States useful and practical information on subjects connected with agriculture, and to promote scientific investigation of agricultural science," experiment stations were to be established. Further on Section 4 provided "That bulletins or reports of progress shall be published" as often as once in three months, and these distributed to farmers and newspapers.

Now the idea plainly embodied in all this was that the station should issue bulletins in order that the farmers might be informed of the nature and results of its activities. This entirely laudable idea worked well enough at first. Very presently, however, as the character and quality of the station work changed and the stations began in some measure to fulfil the second purpose for which they were organized, namely, to contribute to agricultural knowledge by investigation, it came about that bulletins were sometimes issued which, from the very nature of the case, left the farmer, who had the temerity to tackle reading them, on the whole rather worse informed when he had finished than he was before he began. Something of this sort was bound to be the case as soon as experiment station work was of anything but the most superficial character. Just as soon as there began to be issued in bulletin form really scientific papers, of a technical character, it became evident that the publishing activity of a station must perform two separate and distinct functions, and not merely a single one as was evidently contemplated by those who prepared the Hatch Act.

These two functions are: (1) To inform the general public of the activity of the station, with reference to such matters as it (the public) is actually interested in from the viewpoint of practical farming. In other words, one function of station publication is, in the language of the original act, to diffuse among the people useful and practical information. (2) The second function of station publication is

[1] From the Maine Agricultural Experiment Station. This paper formed a part of a symposium on the various forms of station publication at the California meeting of the Association of American Agricultural Colleges and Experiment Stations in the Station Section. The paper was read by Director Charles D. Woods, in the absence of the author.

to promote the progress of agricultural science and stimulate further investigation by making available to workers in that branch of science complete and detailed technical accounts of what has already been done. Technical scientific publication is absolutely necessary to the advance of science. The reason why scientific men publish their results is *not*, as some executives sometimes appear to believe, in order that they may get better jobs, but to contribute their mite to the accumulated and classified experience of the world, which is science.

The distinction between these two reasons for publication was almost entirely lost sight of in the earlier history of the stations. One regrets to observe that even in the present enlightened era of experiment station evolution there does not appear to be universally present a clear and complete discrimination between these two different, and indeed essentially antithetic, sorts of publication. Not very long ago one of our most distinguished directors, in a praiseworthy desire to assert the equality (or even superiority) in respect of intellectual attainment of his constituency as compared with the constituencies of other stations, made a statement in a scientific journal to the broad effect that his institution never had and probably never would put out anything not easily within the comprehension of the worthy and enlightened farmers of his state. Leaving aside as not quite relevant here the fact that such a statement carries *two* implications, one of which I fancy its genial author overlooked in his zeal to make the other, it would seem clear that such a position does not do full justice to the purely scientific function of a part of any station's publications.

The general thesis which I should like to make the text of this paper is that while what has been called above the first function of station publishing activity, namely, the diffusing among the people of useful knowledge, is, on the whole, well served by the bulletin form; on the other hand, the second or purely scientific function of station publishing is, on the whole, badly served by that form. Further, the attempt will be made to show that the best manner of serving the second function, which the whole experience of the world's scientific workers has brought forth, is by publication in established scientific journals.

As a first step towards the establishment of this thesis it is desired to quote a short extract from a most entertaining and famous treatise published nearly four hundred years ago. This work is "A Boke, or Counseill against the Disease Commonly Called the Sweate, or Sweatyng Sicknesse made by Jhon Caius, Doctour in Phisicke. Uery necessary for euerye personne, and muche requisite to be had in the handes of al sortes, for their better instruction, preparacion and defence, against the soubdein comyng, and fearful assaultyng of the same disease. 1552."

John Cajus, or, in its English form Kaye, it will be recalled, was the man who brought about the elevation to the rank of a college of Gonville Hall at Cambridge, since known as Gonville and Cajus College. He was one of the most distinguished physicians of his day, and withal an exceedingly keen, witty and shrewd person, whose great learning never upset his common-sense. In his introduction to the "Sweatyng Sicknesse" he mentions his earliest writings, which were translations out of Latin into English. He goes on to say:

> Sense y^{t} tyme diuerse other thynges I haue written, but with entente neuer more to write in the Englishe tongue, partly because the comoditie of that which is so written, passeth not the compasse of Englande, but remaineth enclosed within the seas, and partly because I thought that labours so taken should be halfe loste among them whiche sette not by learnyng. Thirdly for that I thought it beste to auoide the judgment of the multitude, from whome in maters of learnyng a man shalbe forced to dissente, in disprouyng that whiche they most approue, & approuyng that whiche they moste disalowe. Fourthly for that the common settyng furthe and printig of euery foolishe thyng in englishe, both of physicke vnperfectly, and other matters vndiscretly diminishe the grace of thynges learned set furthe in thesame. But chiefly, because I wolde geue none example or comforte to my countrie men (who I wolde to be now, as here tofore they haue bene, comparable in learnyng to men of other countries), to stande onely in the Englishe tongue, but to leaue the simplicite of thesame, and to procede further in many and diuerse knowledges

bothe in tongues and sciences at home and in vniuersities, to the adournyng of the common welthe, better seruice of their kyng, & great pleasure and commodite of their own selues, to what kinde of life so euer they shold applie them. Therfore whatsoeuer sence that tyme I minded to write, I wrate y^{e} same either in greke or latine.

In this quaint phraseology of three centuries and more ago are stated the fundamental reasons why experiment station workers of to-day will do well to publish the major portion of the purely scientific results of their labors not in bulletins, but in established scientific journals. Point for point, the reasons why learned men should publish their best technical results in the best technical manner were precisely the same in the sixteenth century as they are now in the twentieth. Let us see: The "comoditie" of the station bulletin very rarely indeed passes the compass of America, and consequently fails to get the attention of the European workers in the same field. Secondly, the labors taken in the carrying out of a piece of investigation are indeed more than "halfe loste" if the results are published in a bulletin which chiefly comes to the attention of the farmer, who certainly "settes not by learnyng," in the sense that he is in no wise interested in the technicalities of science.

The third point is one about which we can not perhaps expect full agreement, but as honest differences of opinion can do no harm, let me state clearly as my own conviction, that our friend Kaye is right in his assertion that good work is harmed, and the cause for which it stands is harmed, by so publishing it as to invite the unintelligent criticism of uninformed people. This is exactly what we do whenever we publish technical scientific material in bulletins distributed to the general farming public. In spite of the somewhat rabid admonishings which were directed towards the writer when he made the same statement once before, he ventures now to reiterate that the general agricultural public is, as a class, totally incapable of forming any just opinion of the meaning or value of the technical details of scientific work. To invite them to form and express such opinion merely calls down upon the station and the author ridicule or worse. For those, if there be any such still, whose democracy is so intense as to lead them to the conviction that there are no differences between men, and that the humblest hired man on the farm is the intellectual peer of a Newton or a Darwin, the above will sound undemocratic. It really is not. To preserve peace in the family[2] I am willing to admit that *perhaps* we might all be Newtons had we been subjected to the same environment. My only point is that, whether because of heredity or environment, *in real fact* we are not all Newtons. A page of Sanskrit is, I very much regret to say, totally incomprehensible to me. There are many pages of many bulletins which have been issued by American experiment stations which are totally incomprehensible to most farmers. May we not, then, without calling each other names like "codfish aristocrat," let the matter rest here, and turn to Cajus's fourth point? That point, taken over into our present "universe of discourse," is that since a great deal about agriculture that is purely practical, not scientific in any sense, and of an entirely ephemeral nature, has been and is continually published in bulletin form, it can only work to the hurt of first-class research work, such as nearly every one of our stations is producing, to publish it in bibliographical community with the trivial matter which composes so great a part of bulletin literature as a class. Literary and scientific productions, as well as men, are judged by the company they keep.

The fifth and the "chiefe" reason why the stations should publish more of their research work in journals rather than bulletins is because of the educational value of that method for the station men themselves. If a piece of work is submitted to a technical journal for publication, that work must pass a test of merit which is entirely independent of station politics, executive favoritism, the marital connections of the author, the probable effect on the constituency and next year's appropriation, and a host of other things which have been known to play a part in bulletin publication. The work will be judged by the editorial board

[2] But *only* for that reason!

of the journal strictly on its own merits as a piece of scientific research, and on no other basis. Journal publication provides each director with an opportunity to see the scientific work of his station as others see it. Scientific papers are not unlike favorite sons: it is often very difficult for the fond parent to discern in them any faults at all. Independent editorial boards, on the other hand, do that sort of thing very well.

If an independent chemical, or botanical, or zoological, or bacteriological, or agricultural journal refuses to publish a paper submitted from a station, the author and the director are bound to come to the conclusion, since no other is possible, that in some way or other this paper does not measure up to a standard which disinterested experts in the given field of knowledge regard as the irreducible minimum below which sound scientific work can not fall. On the other hand, if it is accepted the work receives the hallmark of standard character.

There is one objection which has been raised to the publishing of a part or the whole of a station's scientific output in different journals which should receive careful attention. This objection is that by this practise the station's work as a whole does not make the impression of large unity which it does if it is all published in one place, namely, the bulletins of the station itself. A somewhat vulgar expression of this same idea which one sometimes hears is that journal publication makes for the aggrandizement of the author at the expense of the station. It has been a theory of station management in some quarters, though now the theory is conspicuous by its nearly complete absence, that the station as such should alone have visible existence and that the individuals composing the staff (save possibly the director) should be publicly considered as invisible, undiscoverable nonentities, not at home. Journal publication has been considered subversive of this pleasant arrangement.

This theory seems to overlook certain facts of psychology, common sense and ethics. It appears entirely clear that the nearer the actual conditions in a station approach to the theory that the members of its staff are individual nonentities, the smaller is bound to be its measure of glory with its constituency and its peers, quite regardless of its mode of publication. For, after all, a station *is* its staff, *et præterea nihil.*

As a matter of fact, the mere existence of an official institution always suffices to gather to the institution a large part of the *kudos* which may attach to the accomplishments of its component individuals. Plenty of evidence of this, if evidence be needed, is seen in the very small influence an individual can exert except as a member of an organization or institution. And conversely, an institution never gains fame or influence, except through the ability and the achievements of its individual members. The New Paradise Experiment Station is a great station because it has on its staff Dr. J. Doe and Professor R. Roe, who are investigators of great originality and ability, and because its director is a wise and far-sighted man.

To come back to the first point, it is very much to be doubted whether the scattering of the technical publications in journals in any degree detracts from the fame or influence of the station. On the contrary, it is probable that both of these things are considerably increased by this mode of publication. The journals are the standard channel for bringing new results to the attention of the scientific world. They unquestionably reach a much wider audience of scientific men than do the bulletins even under the most favorable circumstances.

So far we have spoken of scientific journals in general. Now let us turn to one in particular, which should interest every station worker in this country, the *Journal of Agricultural Research.* This journal became, almost exactly one year ago, the official organ of this Association and the United States Department of Agriculture jointly. Experiment station papers are received and published on precisely equal terms with department papers. One half of the members of the editorial board are station men. In the editing of the journal the attempt is being made to set a standard as to scientific content and literary form for the papers which shall be as high as the

highest maintained by independent scientific journals, whether in the field of pure or applied science. The journal is being given an extremely comprehensive standard library circulation throughout the world. For the first time it provides a medium of publication, altogether worthy of the best American work in agricultural science.

Will the stations support the *Journal of Agricultural Research* by sending to it specimens of their best output? The past year's experience indicates that the *Journal* meets a real need and will be supported by the stations. Papers have been published or accepted for publication from the following stations: California, Montana, Utah, Minnesota, Illinois, Wisconsin, Michigan, Ohio, Tennessee, Kentucky, both New York stations, Pennsylvania (Institute for Animal Nutrition), New Jersey, North Carolina, Florida and Maine. Which is not a bad showing for the first year!

Altogether it seems to the writer to be inevitable, as the experiment stations take on more and more the character of research institutions, and leave behind more and more that type of activity which was essential at the beginning, but is now being taken over by extension departments, that there will be all the time an increasing proportion of the scientific output published in the standard established scientific journals. In this way only can it take the place which is its due in the world's scientific literature.

Raymond Pearl

THE NAVAL CONSULTATION BOARD

The board appointed by national scientific and engineering societies at the request of the secretary of the navy met in Washington on October 7. Officers were elected as follows:

Chairman, Thomas A. Edison, Orange, N. J.

First Vice-chairman, Peter Cooper Hewitt, New York.

Second Vice-chairman, William L. Saunders, Plainfield, N. J.

Secretary, Thomas Robins, Stamford, Conn.

Assistant to Chairman, M. R. Hutchinson, Orange, N. J.

The board approved a plan for the establishment of a research and experimental laboratory for the United States navy, regarding which a statement was made public as follows:

1. The laboratory should be located on tidewater of sufficient depth to permit a dreadnought to come to the dock. (*B*) It should be near but not in a large city, so that supplies may be easily obtained and where labor is obtainable.

2. The laboratory should be of complete equipment, to enable working models to be made and tested to destruction. There should be: (*A*) A pattern shop; (*B*) a brass foundry; (*C*) a cast iron and cast steel foundry; (*D*) machine shops for large and small work; (*E*) sheet metal shop; (*F*) forge shop for small and large work; (*G*) marine railway large enough to build experimental submarines of 1,500 tons; (*H*) woodworking shops; (*I*) chemical laboratory; (*J*) physical laboratory; (*K*) optical grinding department, etc.; (*L*) motion picture developing and printing department; (*M*) complete drafting rooms; (*N*) electrical laboratory and wireless laboratory; (*O*) mechanical laboratory and testing machines; (*P*) explosives laboratory, removed from main laboratory.

3. The building should be of modern concrete construction, with metal sills and doors, wire glass windows, etc. Ample fire protection.

4. A naval officer of rank should be in charge. He should be especially fitted. (*B*) Under him should be naval heads of broad experience in laboratory methods and science in general—practical as well as theoretical men. They should not go to sea. (*C*) Under them should be staffs of civilian experimenters, chemists, physicists, etc. (*D*) Each sub-head should have his corps of assistants, and with shop facilities, without too much red tape. (*E*) There should be at least two, and possibly three, shifts of men. Time should be the essence of the place.

5. Secrecy should be the governing factor. The place should be surrounded by a high fence and guard maintained at all hours. No visitors allowed.

6. Facilities should exist for enabling the inventor to assist in the development of the idea he has presented, provided he is a practical man.

7. The investment for grounds, buildings and equipment should total approximately $5,000,000.

8. The annual operating expenses to be between $2,500,000 and $3,000,000.

SCIENTIFIC NOTES AND NEWS

THE National Academy of Sciences will hold its autumn meeting at the American Museum of Natural History, New York City, on November 15, 16 and 17.

SECTION II of the Pan-American Scientific Congress will discuss problems of international interest in astronomy and geodesy and in meteorology and seismology. The chairman of this section is Dr. Robert S. Woodward, president of the Carnegie Institution of Washington. It is divided into two sub-sections: astronomy and geodesy, of which Dr. Woodward is the chairman; and meteorology and seismology, of which Professor Charles Frederick Marvin, chief of the United States Weather Bureau, is chairman. Among the topics to be discussed are: (1) The desirability and feasibility of extending a gravimetric survey to cover the American continents. (2) Present condition, needs and prospects of meteorological and seismological work in each of the participating countries of the Scientific Congress. The report from each country should contain a list of all meteorological and seismological stations and other local information pertinent to this report in that country.

TEN additional directors, to represent the important institutions interested in aeronautics, have been added to the executive board of the American Society of Aeronautic Engineers. The appointments are as follows: U. S. Army: Captain A. S. Cowan, commanding S. C. A. S., and Captain V. E. Clark, chief aeronautical engineer, U. S. Army. U. S. Navy: Lieutenant Commander Henry C. Mustin and C. Holden Richardson, naval constructor. Smithsonian Institution: Dr. Albert F. Zahm. Weather Bureau: Professor Wm. R. Blair, in charge of aerological investigation. Bureau of Standards: Dr. D. E. Buckingham. Massachusetts Institute of Technology: Lieutenant Jerome C. Hunsaker, U. S. N. University of Michigan: Dr. Herbert C. Sadler. Aero Club of America: Alan R. Hawley.

THERE has been appointed a British governmental committee to consider and advise on questions of industrial fatigue, hours of labor, and other matters affecting the personal health and physical efficiency of workers in munition factories and workshops. The committee is constituted as follows: Sir George Newman (chairman); Sir Thomas Barlow, G. Bellhouse, Professor A. E. Boycott, J. R. Clynes, E. L. Collis, W. M. Fletcher, Leonard E. Hill, Samuel Osborn, Miss R. E. Squire and Mrs. H. J. Tennant.

DR. JOHN D. BLAKE, Baltimore, has been appointed commissioner of health to succeed Dr. Nathan R. Gorter. Dr. Gorter has been appointed a member of the Maryland State Board of Health.

DR. MILTON J. ROSENAU has resigned as a member of the Public Health Council of Boston and has been succeeded by John T. Wheelright.

BEFORE the Geographic Society of Chicago on October 8, a lecture was given by Dr. Henry C. Cowles, of the University of Chicago, on "Romance and Reality from the Mississippi Bottom Lands."

AT the stated meeting of the New York Academy of Medicine, on October 7, Dr. George W. Crile, of the Western Reserve University, delivered the Wesley M. Carpenter lecture on "Kinetic Drive—Its Phenomena and Its Control."

A TABLET was unveiled, on September 18, in Cheltenham College Chapel, and a life-size portrait by Mr. Hugh Riviere in the College Library, to the memory of Dr. E. A. Wilson, who perished with Captain Scott in the Antarctic. The late Dr. Wilson was educated at Cheltenham College.

SUSANNA PHELPS GAGE, known for her work on comparative anatomy, has died at the age of fifty-eight years. Mrs. Gage received the degree of doctor of philosophy from Cornell University in 1880. She was the wife of Professor S. H. Gage.

WILLIAM HENRY HOAR HUDSON, late fellow of St. John's College, Cambridge, and professor of mathematics at King's College, London, died on September 21, aged seventy-six years.

Professor D. T. Gwynne-Vaughan, professor of botany at University College, Reading, died, on September 4, at the age of forty-four years.

J. U. T. Quensel, professor of pathologic anatomy and hygiene at the University of Upsala, has died at the age of seventy-five years.

Dr. Theodor Albrecht, of the Potsdam Geodetic Institute, head of the International Bureau for Geodesy, has died at the age of seventy-two years.

Among those who have been killed while tending the wounded in the field are G. Heimann, of Berlin, one of the pioneers in eugenics, and Professor H. Piper, of the Institute for Physiology, Berlin. Dr. Piper's work was on the physiology of the senses.

The American Academy of Arts and Sciences receives the sum of $3,000 under the will of William Watson, of which he was secretary. Mr. Watson's scientific books and instruments are to be divided among the academy, Harvard University and the Massachusetts Institute of Technology.

It is stated in *Nature* that at the sale by auction of the Amesbury Abbey estate on September 21, the historic monument of Stonehenge was purchased for £6,600 by a local landowner, Mr. C. H. E. Chubb, of Bemerton Lodge, Salisbury. The estate came into the market in consequence of the deaths of Sir Edmund Antrobus and of his only son, who was killed in action last October. As Stonehenge is under the protection of the ancient monuments act, no steps can be taken by the owner to alter or remove any parts of this remarkable relic of antiquity.

The Botanical Society of Pennsylvania held its twelfth annual scientific assembly in Botanical Hall on Saturday, October 2. The program included illustrated lectures by H. H. M. Bowman and Mr. W. R. Taylor, the former on "Botanical Experiences Along the Keys of Southern Florida"; the latter on "Summer Botanizing on Mount Desert." Dr. Joseph S. Hepburn explained his "Experiments on the Digestive Action in the Pitcher Liquids of Nepenthes."

We learn from *Nature* that the members of the Siberian Expedition sent out sixteen months ago, at the joint expense of the Oxford University School of Anthropology and the University of Pennsylvania Museum, reached London last week. The leader, Miss M. A. Czaplicka, is a native of Russian Poland, and has been a student of the Warsaw University and of Somerville College, Oxford. The expedition consisted of Miss Curtis, the artist, Miss Haviland, ornithologist, and Mr. Hull, of the University of Pennsylvania, ethnologist. They proceeded from Warsaw to Krasniack, in Siberia, and thence to the mouth of the Yenisei. The first tribe examined was that of the Samoyeds, and then the winter was spent among the Tungus of the Tundra, a very primitive race, little influenced by Russian culture. The spring was devoted to the Tartars, who are much more civilized than either the Samoyeds or the Tungus. Much information of scientific interest has been acquired, and a large collection of costumes, weapons, implements, and ornaments made of copper and iron has been made. These will, it is hoped, be exhibited later in Europe and America.

The mid-year review of the copper situation by B. S. Butler, of the United States Geological Survey, records a general betterment in the six months' period. At the beginning of the year 1915 most of the large copper producing companies of the United States had for nearly five months been operating on a 50 to 60 per cent. basis and probably none were producing at normal capacity. A considerable proportion of the smaller producers had shut down their plants, where this could be done without great loss. Developments and improvements had been generally suspended. Copper was selling below 13 cents a pound and had been considerably lower. Wages had been reduced in most of the camps and many men had been either laid off or were employed only part time. Soon after the first of the year, however, there was a notable improvement in the demand for copper and the price

has rather steadily advanced from below 13 cents to about 20 cents a pound, the highest price reached since 1907. With the increase in demand, and the advance in price, there has been a corresponding steady increase in the production of the metal and at the present time most of the larger producers have brought their output to normal, while many of the smaller producers have resumed operations. The output of copper has also probably nearly or quite reached the normal. Wages have been raised in the camps where reduction had taken place and the industry in general is in a highly prosperous condition.

IN his last report to the Union government of South Africa, as we learn from *Nature*, the secretary for agriculture points out that the difficulty of procuring good men to fill the scientific and administrative posts in the department, which has been commented on before, continues. Men of moderate attainments are plentiful and easy to obtain, but good men are more in request than ever. It also appears as if men who are really worth having, and therefore usually in a position to choose, prefer to work in universities and other learned institutions which are independent or semi-independent of government control, or engage in business on their own account, rather than in government departments, as in the former they have more scope and freedom of action and have not to waste time by furnishing multitudes of returns and continually explaining and demonstrating the necessity for their existence. Seeing that the value of the department to the country depends in the first instance entirely upon the quality of its professional and administrative officers, this is a very serious matter. Efforts are being made to overcome the difficulty of obtaining professional and technical officers by giving scholarships to likely young men to study at institutions abroad, at which they can get the best training obtainable in their particular subjects. The course of study is usually a four years' one, and a number of scholars have already returned and been drafted into the department. It is considered that this is one of the best methods of obtaining officers for the department, but it may not entirely suffice, and from time to time officers will have to be appointed from wherever they are obtainable, as at present.

A REPORT of the chief commonwealth railway engineer, gives some details of the progress of construction of the east-to-west transcontinental railway of Australia, according to an abstract in the *Geographical Journal*. It states that the western Australian division survey is complete, and the route has been permanently located to 280 miles. Thence to the border the permanent survey will proceed in advance of plate-laying. The South Australian survey is complete. It is estimated that the rails will be laid throughout before the end of next year, although the rate of progress will be reduced by the very heavy earthworks soon to be taken in hand in the South Australian section. During the three months preceding the date of the report 240 miles had been laid. The line is to be ballasted throughout, and arrangements had been made to select quarry sites and erect the necessary plant for rock-crushing. In view of the scanty water-supply on the route, reservoirs have to be provided at various points, and several are in course of construction. They include one at Karonia, W.A. (late Cardonia), with an approximate capacity of 7,000,000 gallons; one at Bookloo, S.A. (6,000,000 gallons); and one each at Windabout and Eucla (5,000,000 gallons). Boring operations have been carried on in both divisions.

RADIUM deposits, the wearing away of the land by the sea, the make-up of the upper part of the earth's crust at various places, the development of mountain ranges, and the origin of dolomitic limestone are some of the subjects discussed in a volume recently published by the Geological Survey entitled "Shorter Contributions to General Geology, 1914." In former years the announcement of incidental discoveries made by geologists in connection with the study of their main problems has awaited the preparation of extended reports on those problems, but by a plan which has recently been put into operation by the United States Geological Survey such minor

additions to the world's store of knowledge, even though unrelated, are now grouped together in one volume and published as promptly as possible. Some of the conclusions in the volume which has just appeared are of interest to the general public; others will be appreciated only by those who have made a special study of geology. For example, the articles on the rock strata known to geologists as the "Montana group" describe the strata which make up that group and their variations from place to place and interpret the facts set forth, giving their significance as to the origin of the strata and the conditions under which they were formed. Most of the field evidence was obtained in examinations of public land for the purpose of determining its value as coal land. The direct results of such work, those which appeal to the man in the street, are the bringing into the United States treasury of some hundreds of thousands or millions of dollars. Indirectly a thorough knowledge of the strata makes the finding of coal and other valuable deposits easier, but the value of the work is not wholly expressible in dollars and cents, for in the realm of pure science the understanding of the make-up of the earth and its history in the past has a value entirely apart from what such knowledge may at present yield directly or indirectly in money. An article on pitchblende ores of Colorado includes not only an account of those ores in that state but also a brief description of the principal European occurrences of pitchblende, one of the ores of radium. An article on erosion in Chesapeake Bay prophesies that certain islands in the bay will be washed away by the waves within the next century and shows the places on the bottom of the bay to which the sand and soil of these islands is being carried by the waves and currents. Another article describes some lavas which have been thrust into cracks in the earth's crust in the vicinity of Spanish Peaks, Colo. Still another article shows that the echinoderms, a class of sea animals, secrete skeletons of one kind of material in cold water and of another kind in warm water, and that the origin of magnesian or dolomitic limestone which has long been a mystery, may be partly explained by the nature of these skeletons, myriads of which make up considerable parts of certain rocks. Several papers discuss the strata underlying the surface of the earth in various parts of the country and give data of use to the driller of deep wells. A copy of this report—Professional Paper 90—may be obtained on application to the director, United States Geological Survey, Washington, D. C.

UNIVERSITY AND EDUCATIONAL NEWS

Mr. Jacob H. Schiff, a member of the board of trustees of Barnard College and its first treasurer, has given $500,000 to the college for a woman's building. It will include a library and additional lecture halls as well as a gymnasium, a lunch room and rooms for students' organizations.

The University of California has received $100,000 from an anonymous donor to endow the "Dr. C. W. and Mrs. Sarah E. Fox Memorial Beds" in the University of California Hospital, a part of the equipment of the University of California Medical School. These beds are to be maintained in the new University Hospital, now being erected in San Francisco through the gift of $615,000 by various friends of the university. The superior court of San Francisco has just decided in favor of the university a suit for $145,000 brought by the regents against the heirs of John M. Keith, who had refused to pay the balance of $145,000 due under a subscription made toward this new hospital by Mr. Keith, of which but $5,000 had been called for at the time of his death.

The will of the late Anna Yarnall creates a trust fund of $25,000, which is placed in the hands of the trustees of the University of Pennsylvania for the support of the botanic gardens of the Biological Hall at that institution. The income from this trust is to be continued for this purpose as long as the botanic garden is under the supervision of the head of the botanical department.

Since the transfer of the department of geology and geography of the University of Chicago to the new Julius Rosenwald Hall, Walker

Museum has been undergoing the necessary alterations so that it may now be used for museum purposes as was originally designed. The building is being thoroughly repaired, a modern lighting system is being installed, and much material of unique scientific value, which has never before been displayed through lack of space, is now being arranged for permanent exhibition. The director of the museum, which contains more than a million specimens, is Dr. T. C. Chamberlin, head of the department of geology; and the associate directors are Frederick Starr in anthropology, Stuart Weller in invertebrate paleontology, and Samuel Wendell Williston in vertebrate paleontology.

THE University of Illinois is completing arrangements for the construction of a new genetics building. It will contain offices for Dr. J. A. Detlefson and Mr. Elmer Roberts and two laboratories—one for general genetics and the other for animal nutrition with classroom accommodations. When completed the building will be one story in height, 140 feet by 42 feet in width, and will cost approximately $10,000.

BY the will of Mr. W. Jackson, engineer, of Aberdeen, funds are left, subject to his wife's life interest, for the establishment of a chair of engineering in the University of Aberdeen.

PROFESSOR JULIUS STIEGLITZ has been made chairman of the department of chemistry of the University of Chicago to succeed the late Professor John Ulric Neff.

THE Harvard corporation has made the following appointments for the year opening September 27: Dr. John L. Morse, associate professor of pediatrics, has been made full professor; Dr. Frederick T. Lewis, assistant professor of embryology, has been appointed associate professor; Dr. John Warren, assistant professor of anatomy, has been made associate professor; Dr. John L. Bremer, assistant professor of histology, has been made associate professor; Dr. Francis W. Peabody has been appointed assistant professor of medicine and Dr. Herbert S. Langfeld, assistant professor of psychology.

APPOINTMENTS in the department of agronomy at the Iowa State College for the year include: Ross L. Bancroft, M.Sc. (University of Wyoming and Iowa State College), assistant professor of soils; H. W. Johnson, M.Sc. (Iowa State College), instructor in soils and assistant in soil bacteriology; F. S. Wilkins, M.Sc. (University of South Dakota and Iowa State College), instructor in farm crops, and Roy Westley, B.Sc. (Iowa State College), instructor in farm crops.

PROFESSOR A. B. PLOWMAN, PH.D. (Harvard), has taken up his work as head of the department of biology, in the Municipal University of Akron, Ohio.

PROFESSOR WILLSTAETTER, member of the Kaiser Wilhelm Institute for Chemistry, has been made professor of chemistry at the University of Munich.

DISCUSSION AND CORRESPONDENCE

POTASSIUM FROM THE SOIL

BULLETIN 182 of the Illinois Experiment Station by Hopkins and Aumer, brings, under the above caption, the results and discussions of a three-year course of experimentation in the growing of crop plants in the "insoluble residue" left after digestion, according to the "official method," for ten hours in HCl of 1.115 sp. g., of a "normal" soil from the Illinois corn belt, of good productiveness. The authors recall that in bulletin 123 of their station it had already been shown that this method of digestion extracted only 15 to 25 per cent. of the total potassium present, as determined by the method of fusion. In the present series of tests it was clearly shown that red clover was able to take from the insoluble residue sufficient potassium to supply a normal crop, so long as nitrogen and phosphorus were adequately present; thus illustrating the futility of the "official method."

It seems proper now to recall to mind that in the early seventies, Loughridge at my suggestion made an elaborate investigation of the effects of the digestion of a "normal" soil with

acids of different strengths, and for different times. The results of this investigation were published in 1873, in the *American Journal of Science,* and in the *Proceedings* of the American Association for the Advancement of Science, having been read before that association. It was conclusively shown that there was a steady increase in the extraction of potassium for *five* days, remaining stationary afterwards, the amount extracted during the first twenty-four hours being about one half of the final figure, while phosphorus, lime and magnesia were fully extracted.

Notwithstanding this demonstration, fully published in two standard publications, a number of years later the "Official Chemists," in a meeting at Washington, hastily adopted, against my protest, the arbitrary ten-hours digestion proposed by Kedzie, as the official method to be used in state and government work.

It is no wonder that as a result of this irrational practise, chemical soil analysis became more and more discredited as a means of ascertaining the quality and permanent productiveness of soils. In cases where potassium was in abundant supply, it gave results corresponding to the field tests because of the complete extraction of phosphates, lime and magnesia during the ten hours' digestion. On the other hand, where potassium was deficient, no definite relation between the analysis and practise *could* appear.

But when Hopkins goes so far as to determine the potassium content by the fusion method, thus decomposing all the resistant silicates, feldspar-sand, etc., as well as the easily decomposable zeolitic minerals, he goes far beyond the limits within which any definite correlation between soil composition and vegetative action is to be expected; and whatever conclusions are based upon such analyses are practically groundless. Knowing as we do that the assimilation of inorganic substances from the soil by plants is mediated by *acid* solvents, whether derived from the air, from vegetable decay, from secretion by plant roots or bacteria, it certainly is most rational to ascertain how far *acid* action can go in the soils under examination. *This* limit, and no arbitrary rule of time, or ultimate analysis, must serve as the basis of judgment for practical comparison of soil values, or producing capacity. Hopkins's own experiments on the growth of plants in the undissolved residue from the "official" analysis simply corroborate what had been abundantly shown by Loughridge's work in 1873, but prove nothing against the practical value of soil analyses properly made. They do throw discredit upon the "official method," so far as potassium is concerned.

But soil chemists would feel additionally indebted to Hopkins if he would undertake to supplement the somewhat gratuitous proof he has given of the inadequacy of the official method, by growing plants on the residue from a digestion carried to the limit of acid-solubility; which in the case of the soil selected by Loughridge and myself we found to be five days for acid of the accepted sp. g. of 1.115. I have long desired to make this crucial test, but have not been able to find the time or means to do so. If an Illinois soil can thus be made to yield to any plant a practically important amount of potassium, it will be very desirable to know it and thus put an end to farther controversy in the matter; while rendering an important service to soil investigation and plant physiology.

E. W. Hilgard

University of California,
September 10, 1915

ELEMENTARY MECHANICS

To the Editor of Science: There have appeared in your pages recently a number of contributions by various authors to the discussion of the dynamical equation $ma = f$ or some of its possible variants. It seems as though it would be necessary, for a complete discussion of the relative merits of the different ways of introducing a student to the dynamical equation cited, to enter at least briefly upon the matter of the student's previous training in mechanics. We are all aware that it is at present somewhat stylish to begin the study of mechanics with kinetics and to treat statics as a special case in which the accelerations are

zero, and impact as a special case in which large forces act through small periods of time. This, however, is a distinctly recent movement. The older method of procedure was to study, first, statics and problems in impact and thereupon to proceed to kinetics. The reason for this order was probably not wholly logical but largely pedagogic or historical. A student who has a small knowledge of trigonometry is quite fitted, mathematically, to study both statics and problems in impact; whereas, to obtain valuable training in kinetics a knowledge of the differential and integral calculus, including the simpler differential equations, is necessary. Moreover, as a matter of history, statics and impact precedes, I believe, kinetics. Let us suppose that the student has followed this historic and pedagogical order. In his statics he will have learned to deal with forces; these forces may be measured in any units that are convenient, provided only that all the forces are measured in the same units in the same equation; for the equations of statics are homogeneous in the forces. (I, of course, am speaking only of elementary statics, not of the theory of virtual velocities or of potential energy.) In studying impact the fundamental conception is that of momentum. The student learns that momentum is the product of mass by velocity; that momentum is resoluble as are forces; and that in impact the momentum of a system is conserved. He is then in a position to solve problems in inelastic impact of particles and, with an additional simple law concerning relative velocities, he can proceed to elastic impact. In the problems in impact the units of mass may also be anything, provided, again, that they are the same for all masses; for here again the equations are homogeneous in the masses.

When, now, such a student comes to kinetics he is able at once to proceed to Newton's second law, namely, that the rate of change of momentum is equal to the force. Here, however, we have an equation which is no longer homogeneous either in the mass or in the force, and it is evident, or can be made so to any student, that he can not use arbitrary units of mass and force, but that the two units must be in some way correlated. Indeed we should state the second law in the Newtonian form: The rate of change of momentum is proportional to, or varies as, the force. We then write

$$\frac{d}{dt}(mv) = kf.$$

The constant k, like any factor of proportionality, is determined by substituting the known values for some special case. We naturally select the simplest; that is, a mass falling under its own weight. If now we measure mass in pounds, as we (probably) did in the theory of impact, and force likewise in pounds, as we (likely) did in statics, we find that the mass of weight W has, under the force of weight W, an acceleration g; hence

$$\frac{d}{dt}(Wv) = Wg = kW$$

or $k = g$. We therefore have the fundamental equation of kinetics in the form

$$\frac{d}{dt}(mv) = gf.$$

If we desire to use some other system of units for mass and force we should likewise have to determine a constant k.

It is, of course, true that a weight is not a definite constant thing from place to place, but I should not think of calling the student's attention very vigorously to this difficulty at this stage, particularly as it again is no difficulty at all, provided mass and force are both measured in weights at the same place. Nor do we need to mention that the equation which involves the momentum is one which can still be regarded as valid when the student reaches the theory of relativity and modern electrodynamics whereas the equation $ma = f$ or any equation involving accelerations leads to the ridiculously needless concepts of transverse and longitudinal (and an infinity of oblique) masses.

It has always seemed to me that the historic and pedagogical method of procedure was still the best, notwithstanding the above mentioned and modern style. It is quite true that from a logical point of view things proceed more simply when we start with kinetics; but logic

is a very poor substitute for common sense, and it is probably logic more than anything else that makes trouble with our pedagogy in mathematics and, even more, in mechanics and physics—perhaps one would hardly try to be logical in theoretical chemistry. Or let us put it another way. There are various kinds of logic; one kind the mathematician's, which to a certain extent is adopted by others; the other kind of logic being the logic of everybody else; a biologist probably has a logic very different from that of the mathematician and very much more useful to him.

From the pedagogical standpoint strict logic, with all its beauties (which the student always misses) is the most illogical thing there is. The important thing for the student and his teacher is to keep as close to every-day life as possible, and any student knows what a weight of 4 pounds is, so that he can proceed to statics. Moreover, he finds no difficulty in measuring the mass or "quantity of matter" by weighing it, so that again he can proceed to problems in impact. The philosophy of mass or force will appeal to him much more after he knows something about mechanics. Our first problem is to get the student into a position where he can solve such simple problems in mechanics as he sees in the actual world on every side about him, and a certain amount of ignorance, which would be very lamentable on the part of myself and your other contributors, is highly praiseworthy in the student.

Edwin Bidwell Wilson
Massachusetts Institute of Technology

THE END IS NOT YET!

Mr. Gerald H. Thayer in a communication to Science for September 3, 1915, claims to have disposed of Cory's Shearwater, *Puffinus borealis*, by establishing it as a synonym of *P. kuhli*. He finds this identity first claimed by Saunders and later, finding that Godman in his "Monograph of the Petrels" takes the same view of the relationship of the two birds, he considers the matter settled for all time, adding: "It would seem unnecessary, not to say presumptuous, for us to question this determination, or wait to make further comparison of specimens." Ornithology would be in a sad state if we accepted all statements without attempting verification, and fortunately others have not regarded further investigation in this instance as "unnecessary" or "presumptuous."

Had Mr. Thayer looked into the matter a little more fully he would have found that in the *Ibis* for July, 1914, Mr. D. A. Bannerman questions the correctness of Saunders's and Godman's treatment of *Puffinus borealis* and later[1] he affirms its distinctness. Furthermore, Mr. Bannerman was, quite naturally, struck by the fact that the type of Gould's *flavirostris* came from the "Cape Seas" while the bird to which the name was applied by Hartert was a native of the Azores and other east Atlantic islands. Mr. Thayer passed this matter over without investigation, but Mr. Bannerman upon comparing topotypes of *flavirostris* with the Azores bird found that they represented two different forms and named the latter *fortunatus*. Now the interesting point in all this is that should the bird from our north Atlantic coast be regarded as identical with the Azores form the name *Puffinus borealis* Cory is the oldest name for it and must be used; while if they are regarded as distinct, then the American bird will still be known by Cory's name. In either case we shall retain Cory's Shearwater on our list!

Mr. Bannerman regards all these shearwaters as subspecies of *P. kuhli*, but this does not affect the distinctness of the forms, as the difference between a species and subspecies is not one of degree of difference, but of the presence or absence of intergradation along the line separating their ranges. It must in many cases be largely a matter of opinion, which rank a given form should take. Hasty action like that of Mr. Thayer's, without the examination of adequate material, is responsible for much of the shifting of names back and forth which has become such an abomination in modern systematic zoology.

Witmer Stone
Academy of Natural Sciences,
Philadelphia, Pa.,
September 4, 1915

[1] *Bull. Brit. Ornith. Club*, May 26, 1915.

THE "PAN-AMERICAN SCIENTIFIC CONGRESS"

TO THE EDITOR OF SCIENCE: You will pardon me if I desire to draw your kind attention to the term "Pan-American Scientific Congress" as applied to the congress which, according to the last issue (No. 1,083) of SCIENCE, is to meet in the city of Washington next December. Scientific bodies are generally understood to represent bodies dealing with science or exact knowledge. Now, inasmuch as geography is a science, and geographical science teaches us that the continent of America includes lands from the northernmost tracts of British America to the southernmost areas of Patagonia, the term "Pan-American" can not be properly applied to any scientific congress, body or society which does not include all the countries and lands of the continent of America.

H. A.

SCIENTIFIC BOOKS

Handbook of Medical Entomology. By WM. A. RILEY AND O. A. JOHANNSEN. Ithaca, N. Y., Comstock Publishing Company, 1915. Pages 1 to 348. Figures 1 to 174.

The writing of a book on a subject to which so many important contributions are being made as to medical entomology is not an easy undertaking. The author is likely to find, when he lays down his pen at the end of a chapter, that an article has appeared which makes it necessary for him to revise his statements in many important particulars. The writers of this book are both successful teachers and the experience they have had in the class room has been brought into play in the manner of presentation of the subject. As a matter of fact six years of teaching medical entomology is undoubtedly the best possible preparation for the writing of such a volume. Consequently it is not surprising, at least to those who are familiar with the work of Drs. Riley and Johannsen, that their "Handbook" is a very clear and logical treatment of the subject with which it deals.

The division of the subject into topics treated in separate chapters is most commendable. The directly poisonous species, the accidental parasites, the simple carriers of disease, the direct inoculators of disease germs, the essential hosts of pathogenic organisms, and other groups are thus treated. The method is undoubtedly more satisfactory from the standpoint of the student than the one followed in many works on the same subject which divide the matter on the basis of the diseases transmitted. Of course it is important to consider the latter phase of the subject and this is done in the series of chapters following those dealing with the different classes of insect transmission of diseases.

The judgment of the authors has been exercised in the discussion of such diseases as poliomyelitis, pellagra, verruga, and others in which insect intervention in any important way has not been fully established. Thus they pursue a conservative course and one which must be beyond criticism by those who are inclined to minimize the importance of insect transmission of diseases.

The last part of the work includes taxonomic tables dealing with ticks, flies, bugs and other insects which are concerned in the transmission of diseases. This is an essential part of the book and will serve as a basis for the work of students for many years.

That the book is up to date is shown by the fact that though the preface is dated January, 1915, it includes, as an appendix, an important article by Stokes which appeared in a medical journal for the month of December, 1914.

The bibliography will be found most useful, although some important works, like Howard's book on the house fly, and a number of articles to which references are made in the early text, are not included.

Recently the center of interest in medical entomology has been England, and the fact that the work of Smith and Kilbourne on splenetic fever in this country, of the American Army Commission which investigated yellow fever in Cuba, and of Ricketts on spotted fever, helped to lay the foundation of our knowledge has to some extent at least been overlooked. The "Handbook" places the relative contribution of different agencies in a clear light but its most important function will undoubtedly be to stimulate interest in

further investigations and to supply a reliable and much needed aid. W. H. Hunter

U. S. Department of Agriculture

Abwehrfermente. Das Auftreten blutfremder Substrate und Fermente im tierischen Organismus unter experimentellen, physiologischen und pathologischen Bedingungen. Von Emil Abderhalden. Fourth, considerably enlarged edition. Published by Julius Springer, Berlin, 1914. Pp. xxiv + 404; with 55 text-figures and four plates.

In the fourth edition of this book, which first appeared about two years ago as a modest pamphlet, especial stress has been laid upon the necessary technique for demonstrating the specific ferments which form according to Abderhalden when any body-alien, tissue-alien or blood-alien proteid, carbohydrate or fat is brought into intimate contact with the tissues of an animal organism.[1] Numerous drawings accompany the text and detailed instructions are given for the various preparations and manipulations which must always be carried out with rigid aseptic precautions and with adequate controls. Sources of error are exhaustively treated and indeed are so numerous that perhaps any failure could be explained by some slip in technique. This technical part occupies one half of the book, the other half being devoted to an exposition of the theory and its numerous stimulating corollaries.

It is unfortunate that the method has not been simplified, for its difficulty is probably the main cause of the disagreement which still exists among competent investigators about the availability of Abderhalden's methods in the serodiagnosis of organic functions.

The widespread attention which Abderhalden's important work has aroused is well shown by the appended bibliography, which, though incomplete, numbers more than 300 titles.

The book is written with expository skill and with charm, and will be read with interest and profit even by those who are in scientific disagreement with its teachings. John Auer

Rockefeller Institute

[1] See the review of the second edition, Science, 1913, N. S., XXXVIII., No. 988, p. 820.

Sun Lore of All Ages. By William Tyler Olcott. G. P. Putnam's Sons. 1914. Pp. xiii + 346. Illustrated.

The setting of the dimmed sun in the west at night and its rising, refreshed and glowing, in the east on the following morning, presented a mystery to the early peoples of the world: to the dwellers in ancient Egypt, to the Incas of Peru, and to the Indians of our western plains. This mystery has been solved in many ways and has given rise to numberless legends, traditions and superstitions. These traditions Mr. Olcott has traced, the legends and superstitions he has collected and compared, and has formed the whole into a very readable and attractive book. The work, which is a worthy successor to the author's "Star Lore of All Ages"; is well printed, beautifully illustrated, and forms an attractive addition to any library.

Chas. Lane Poor

HEMOGLOBINOPHILIC BACTERIA

The hemophilic or more properly hemoglobinophilic bacteria comprise a rather large group of bacilli which grow only in an artificial medium containing hemoglobin. This group does not include the many bacteria that, while growing better in media containing blood or blood serum, will also grow in media not containing hemoglobin. Its representative organism and by far its most important member is the influenza bacillus (*B. influenzæ*) which was discovered by Pfeiffer (hence commonly called Pfeiffer's bacillus) in the respiratory tract of patients afflicted with influenza during the great pandemic in 1889–90. Not only did he discover and isolate this organism at that time but he definitely proved its hemoglobinophilic character a property of bacteria hitherto unknown.

In his classical paper[1] in which he reported these researches he also described other organisms differing in certain respects from the true influenza bacilli, but similar in being hemoglobinophilic. These he called pseudoinfluenza bacilli. Since then these pseudo forms, which

[1] *Zeit. f. Hygiene,* 1893, 13, p. 357.

have also come to be referred to as influenza-like bacilli, have been found especially in the upper respiratory tract in a great variety of diseases. They are frequently met with, for example, in measles, whooping cough, bronchitis, diphtheria, chickenpox, pulmonary tuberculosis, bronchiectasis, pneumonia, and occasionally, as the writer has shown, in apparently normal throats. These bacilli are all very similar, indeed, practically identical, and more recent work seems to indicate that they are also probably identical with the true influenza bacillus differing from it only perhaps in respect to virulence.

Bacilli of this group vary greatly in this respect. As an illustration may be cited the organisms of this type which not uncommonly cause acute meningitis in children. They are found in immense numbers and often in pure culture in the spinal fluid and in the meninges of such patients. They seem to be identical with the true influenza bacilli, having, however, a much higher degree of virulence for animals. In rabbits, for example, after inoculation they may produce death by true septicemia, a result usually not possible to obtain with ordinary doses of Pfeiffer's bacillus.[2]

A further point of interest in connection with all the above bacilli is the fact that when grown on media in the presence of other bacteria, for example, streptococci, staphylococci, etc., they multiply more rapidly, their colonies are larger and their virulence for animals is increased. In other words, they clearly show, to a marked degree, the property of symbiosis.

Several other varieties of hemophilic bacilli differing in certain respects and especially in relation to symbiosis have been described by a number of observers. Friedberger[3] found in the preputial secretion of dogs such an organism, a very minute, gram negative, non-symbiotic, non-pathogenic bacillus. Several years ago the writer[4] described a somewhat similar bacillus isolated from the pathological urine, in three patients in which there was evidence that it had a causal relationship to the infectious process, and since then C. Koch[5] has described an identical bacillus which he believes to have been the causal organism in a number of cases of puerperal infection.

Recently the writer obtained a bacillus from a large abscess of the shoulder joint in an infant a few months old. Not only cultures of pus from the abscess obtained by aspiration, but also cultures of blood obtained from the median basilic vein gave a pure growth of the minute bacillus which was strictly hemoglobinophilic and which resembled closely the influenza bacillus in all respects except in its symbiotic property. The same bacillus was grown from the bronchial secretion and it was probably from this source that the organism first entered the circulation and later localized in the shoulder joint.

In the literature there are a few other isolated instances where bacteria of this general type have been encountered.

These bacilli are interesting in that they are pure parasites, for the very evident reason that only in animals can they find the hemoglobin which, so far as we know, is absolutely necessary for their existence. And since they are not spore formers and are all very delicate organisms, their length of life outside the animal body is very short, probably a few days at the most.

The rather remarkable and extreme adaptation which they have undergone in relation to hemoglobin is also an interesting and important biological phenomenon. While hemoglobin seems indispensable for their growth certain closely related respiratory pigments, for example, hemocyanin and hemerythrin, which occur in the blood of some of the lower animals and appear to have a function similar to hemoglobin in the higher forms, can not be utilized.[6]

The exact rôle which hemoglobin plays in their metabolism is not known. They seem to be able to use this substance about equally

[2] Cohen, *Ann. de l'Inst. Past.*, 1909, XXIII., 273.

[3] *Cent. f. Bakt.*, I., 193, Orig. 33, p. 401.

[4] *Jour. of Infectious Diseases*, 1910, 7, 599.

[5] *Zeit. f. Geburtsh. u. Gynäkol.*, 1912, LXIX., 634.

[6] Davis, *J. Inf. Dis.*, 1907, 4, 73.

well from nearly all the higher forms of animal life, though the hemoglobin from the pigeon as a rule gives a somewhat more abundant growth. It is doubtful whether the hemoglobin is necessary on account of its nutritive properties, because extremely minute quantities in media suffice for growth. The phenomenon may be, therefore, a catalytic one; but further study along this line is needed to prove this point.

There are other points concerning these bacteria which need further investigation, for example, the phenomenon of symbiosis above referred to. In this regard many bacteria occurring in the lower animals should be studied and we should also study and record more thoroughly than has been done, the properties of the non-pathogenic bacteria in this respect.

This group of organisms it seems to me has not received as much attention as it deserves by bacteriologists in general. To illustrate this, I might call attention to the fact that in the very excellent and serviceable descriptive chart for bacteria prepared by the Committee on Methods of Identification of Bacterial Species and endorsed by the Society of American Bacteriologists no provision has been made for recording the properties which bacteria manifest toward blood. This not only applies to the group of hemophilic bacteria but also to many bacteria which have the property of hemolyzing blood and therefore commonly called hemolytic bacteria. Hemolysis is an important characteristic of certain bacteria, for example, streptococci, cholera vibrios, etc., and being fairly constant and quite readily determined by several methods it has come to be of real practical value in the identification and differentiation of organisms.

David John Davis
University of Illinois

SPECIAL ARTICLES

ARTIFICIAL DAYLIGHT FOR THE MICROSCOPE

An examination of the laboratories for students, investigators and private workers with the microscope in our country will show that a very large number can not employ daylight, but must depend on artificial light, although increasingly in biology and pathology stains of all shades and combinations are used to color the objects studied to bring out their structural details.

As daylight is the form of light for which the human eye was developed in the course of its evolution, and as it is the only light which gives to the eye the true color values of the objects in nature, and the multitudes of artificially colored objects in the industries, arts and sciences, naturally many efforts have been made to render artificial light more like daylight.

The accompanying diagram shows very strikingly the difference between daylight and the light from a nitrogen-filled tungsten lamp. The lamp-light is *relatively* too strong in all the colors beyond the violet, and the difference becomes very great in the green and the red. In the other artificial lights commonly used, except the arc, the difference from sunlight is even greater.

As can be readily seen, in order to render any artificial light like daylight, the values of the various colors of the spectrum must be like those of daylight; and this can be attained only by reducing the excess of the red, green and other colors in the spectrum of the artificial light in such proportion as to make the energy curve of its spectrum like that of the sun.

Until very recently all the efforts to make a light filter or screen for artificial light which would transmit light having daylight qualities by which colors could be detected and discriminated with the same certainty as in daylight, were unsuccessful.

During the last two years Dr. Henry Phelps Gage, working in the laboratories of the Corning Glass-works, with the facilities there found, has developed a glass filter which renders the light from a nitrogen-filled tungsten lamp almost exactly like daylight.

In his own words:

> The investigation was started with the idea that a very close approximation to the theoretical requirements would be necessary, and the results have justified the belief that the most perfect approximations are the best.

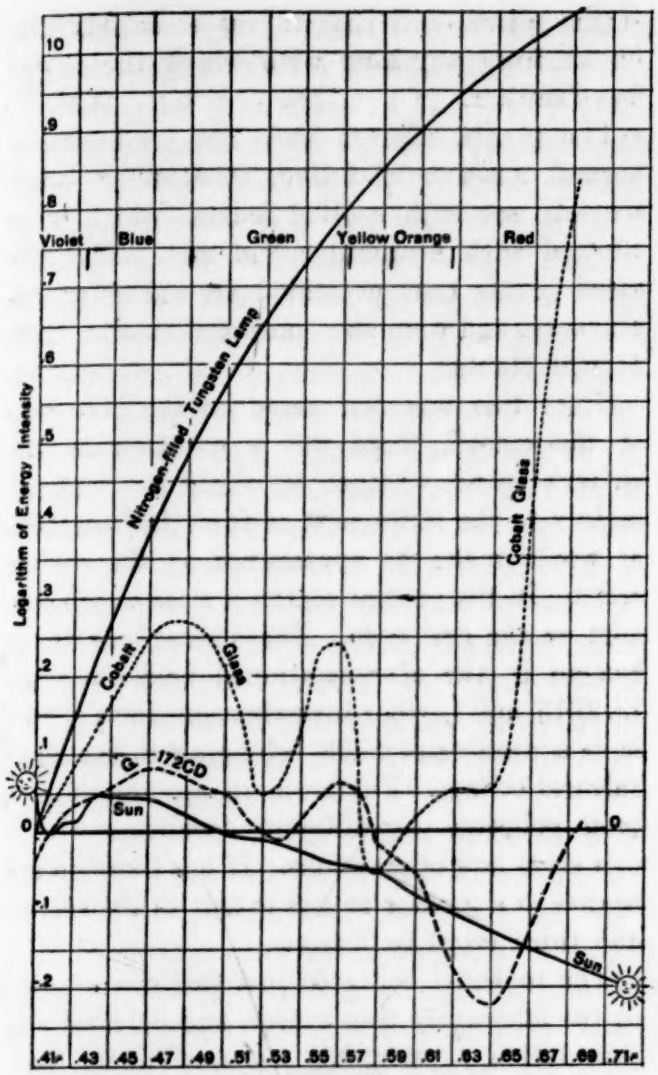

DIAGRAM SHOWING THE DISTRIBUTION OF ENERGY IN THE VISIBLE SPECTRUM OF SUNLIGHT AND OF THE LIGHT FROM A NITROGEN-FILLED TUNGSTEN LAMP; also of the lamp-light filtered through cobalt glass and through the daylight glass (G.172 CD).

The accompanying diagram shows the distribution of energy in the spectrum of sunlight and of the nitrogen-filled tungsten lamp; it also gives the curve of the light from the nitrogen-filled tungsten lamp filtered through cobalt glass and through the daylight glass, 172 CD.

The curve for the light filtered through the daylight glass approximates very closely to that for sunlight, especially between wavelengths .45μ and .65μ, that is, in the region of the visible spectrum giving the greatest amount of useful light.

While light filtered through the proper thickness of cobalt glass may look white to the eye, it gives even more imperfect color values than the unfiltered artificial light. This is intelligible from the violent irregularities of the curve of the light through cobalt glass, and especially the enormous excess in the red as shown in the diagram.

Light filtered through the daylight glass has been very critically tested in my laboratory with microscopic objects stained with many different dyes, and some of them with several dyes on the same specimen to differentiate the various structural details in the same organ. To make sure that the microscope modified in no way the color values, apochromatic objectives and compensation oculars were used as well as the achromatic objectives and Huygenian oculars.

The tests were made in the daytime by a window so that it was possible to turn the mirror from the artificial daylight to true daylight instantly and to determine any difference in appearance, if such difference existed. It was impossible to detect any difference in the colors, although tests were made with the most varied specimens and with a full range of objectives, including the 1.5 mm. oil immersion.

Not wholly trusting my own judgment, I secured that of colleagues in histology and embryology and microchemistry from our own and five other institutions, and their judgment entirely confirms my own.

In practise it was found desirable to have the daylight glass finished with the ground or velvet surface on one or both sides, and to place it in the opening of an opaque screen between the artificial light and the microscope. With this arrangement of the light, the effect is like that from a white cloud.

As stated above, this glass filter was designed to give daylight qualities to the light from a nitrogen-filled tungsten lamp, and gives the most perfect and satisfactory illumination for the microscope with this lamp. To thoroughly test the glass, the other light sources used in microscopic work were also tried; viz., the vacuum tungsten, and the carbon-filament

lamps, illuminating gas with welsbach mantle, acetylene and finally the flat-wick kerosene flame. None of these other sources gave exactly the same color values as daylight. However, the approximation to daylight was surprisingly good, and the worst one, *i. e.*, the kerosene flame, gave better color values than the best artificial light without the color screen. The original intensity of the carbon-filament lamp and the kerosene flame is not great enough to give the best results with the daylight glass. This is because much of the artificial light must be absorbed to render it like daylight.

To the writer it seems that this glass must be a great boon to all those who must use the microscope with artificial light. As the light is soft like that from a white cloud, the comfort to the eye is most gratifying; and of almost equal importance, it gives certainty in distinguishing the most delicate colors and the various combinations of colors. It seems, furthermore, to promise great help in the textile and dye industries, in chemistry and in medicine, as it offers a standard daylight without the changes of real daylight depending on whether the sun is shining or whether the light is from the blue sky or from a cloudy sky. And finally it is believed from the experiences of the writer, that it will furnish great relief to those with sensitive eyes who must work by lamp-light, as it renders the light soft and agreeable like the most favorable daylight.

Simon H. Gage
Laboratory of Histology and Embryology,
Cornell University

A NEW ALFALFA LEAF-SPOT IN AMERICA

While passing an alfalfa field in the vicinity of Manhattan, Kans., in October, 1914, the writer's attention was attracted by the irregular stand, which was noticeable from the road. Since light frosts were rather frequent at the time of year, the condition of the alfalfa was at first thought to be due to these, but closer examination indicated that these could not be the cause. Careful investigation showed that a leaf-spot was prevalent on many of the plants, and that it was strikingly different from anything with which the writer was familiar.

The plants affected were not producing a normal amount of foliage, the stems being sparsely set with spotted leaves, which were affected with a singular leaf-spot. The diseased plants thus presented an unthrifty appearance, and were also somewhat smaller than normal plants.

Since this was discovered on the last crop of the season, there was a question in the writer's mind whether it would be confined mainly to the latter part of the alfalfa season, or would make its appearance on the earlier crops. In the present season a close watch was kept on the first crop. The disease was again located in the aforementioned field on April 17, 1915, and further investigations have shown it in a number of fields belonging to the agricultural college. Furthermore, specimens have been collected in different localities within this state and other states. It has likewise appeared to a greater or less extent in the second and third crops in Kansas.

The material collected last fall was studied in the laboratory this winter, and cultural and inoculation experiments are now under way. There is no question as to its pathogenicity. The fungus is an ascomycete, the perfect stage (perithecia) being found in the mature spots. It belongs to the genus *Pleosphærulina*. The writer has been unable to locate any literature pertaining to its occurrence in America. The species has not been definitely determined. An alfalfa leaf-spot due to *Pleosphærulina Briosiana* Pollacci has been reported by Pollacci,[1] Bubak[2] and Puttmans,[3] in Italy, Austria and Brazil, respectively. There is some doubt, however, whether the species with which the writer is working is *Briosiana* Pollacci,

[1] "Spora una nuova malatta deil erba medica," *Atti del Instituto Botanico, dell' Universita di Pavia,* Nuova Serie, Vol. II., Serie 1901.

[2] "Eine neue Krankheit der Luzerne in Osterreich," Wiener Landwirtschaftliche Zeitung, Nov. 20, 1909, Nr. 93, s. 909.

[3] "Diseases of Cultivated Plants," *Revista Agricola Sao Paulo,* Nos. 114–125, pp. 379–381, 1905.

since Bubak,[2] in his description of this alfalfa leaf-spot, states that a perithecium does not contain more than three or four asci; while, in the material at hand, the number varies from 8 to 14. Furthermore, the ascospore and ascus measurements do not agree entirely with those given by Pollacci,[1] or Bubak. Pollacci first reported and named the fungus in Italy. He does not give the number of asci in a perithecium, and his ascospore and ascus measurements do not agree with those by Bubak, although the latter regards the species as the same. Puttmans[3] describes a variety, naming it *Pleosphærulina Briosiana* Pollacci var. *Brasiliensis* Puttmans nov. f. He regards it as different from *Pleosphærulina Briosiana* Pollacci, in that the ascus and ascospore measurements are larger. Among the seven species under the genus *Pleosphærulina,* described by Saccardo,[4] including *Briosiana,* nothing further is elucidated.

A description of this alfalfa leaf-spot as it occurs in Kansas is as follows: The spots are scattered irregularly over the entire leaf surface, frequently causing spots along the margins. These spots are generally circular or elliptical, from 1 to 5 mm. in diameter. During the earliest perceptible stages, the spots appear as very small, dark-reddish-brown spots. These soon increase in size, a dark-brown margin bounding the ashen-gray center of the spot. The centers of these spots may vary from a light tan color to ashen-gray. This tissue does not fall out, but remains intact. The spots are confined almost exclusively to the leaves, but the fungus does attack the petioles. The perithecia are visible to the unaided eye if they are mature, appearing as very small black dots. They occur rather sparingly, irregularly and promiscuously scattered within the centers of the spots. They are more or less membranous, partially immersed, erumpent, globular to oblong, slightly pyriform, glabrous, dark brown to black, 100–120μ in diameter. The asci are ovoid in shape, varying from 8 to 12 in number, and measuring 56–75μ long, and 38–42μ wide. There are no paraphyses, which fact distinguishes this fungus from the genus *Catharinia.* The asci are supplied with a pedicel at the base, with which they are attached to the wall of the perithecium. Each ascus is provided with a peculiar tongue-like projection at the apex, this being a striking characteristic. This does not appear to be described or mentioned, so far as the writer has been able to find, in Pollacci's description of this fungus. The ascopores measure 12–14μ wide, and 30–32μ long. They are generally arranged so that 5 ascospores are located at the base and larger end of the ascus, and 3 in the upper or narrower part. The spores are multicellular, oblong, fusiform, conspicuously granular, and greenish-hyaline, and having from 3 to 4 septa, and from 1 to 3 longitudinal divisions, making from 5 to 7 cells, rarely 8.

This leaf-spot may prove to be of considerable economic importance, since like the *Pseudopeziza* leaf-spot, it causes destruction of the foliage.

LEO E. MELCHERS
DEPARTMENT OF BOTANY,
KANSAS STATE AGRICULTURE COLLEGE

[4] Sylloge Fungorum., Vol. XI., XIV., XVI. and XVII.

DIFFERENTIATION OF WANDERING MESENCHYMAL CELLS IN THE LIVING YOLK-SAC

THE yolk-sac of the teleost egg is a particularly favorable object for observing the movements and migrations of cells in the developing embryo. Such a yolk-sac has only one really definite continuous membranous cell layer, the ectoderm; a true endodermal layer is absent, though a superficial syncytium, the periblast, fuses with the actual yolk surface. The mesodermal layer is represented by numerous separate wandering mesenchymal cells. These freely wandering mesenchymal cells may be clearly observed through the perfectly transparent ectoderm as they move over the surface of the periblast.

The writer has attempted a detailed study of the movements of the mesenchyme cells and their manner of development and differentiation on the yolk-sac. Observations have been made on the normal embryos from the earliest stages at which the mesenchyme wanders out upon the yolk up to the late embryo in which a com-

plex vitelline circulation is fully established, and all of the products of the yolk mesenchyme completely differentiated. The study has been greatly facilitated by a comparison of the normal embryos with specimens in which the circulation of the blood was experimentally prevented from taking place. In such specimens the cells on the yolk-sac never became confused or contaminated with other cellular elements introduced by the circulating blood. The wandering cells may thus be completely followed through all stages in their isolated position.

In the first place, I can not resist the impulse to highly recommend that all students of hematogenesis spend some time at least in a study of living mesenchymal cells and their histogenesis. Such a study will soon convince one of the great disadvantages under which an investigator labors in attempting to solve the origin of blood from observations on dead material in serial sections. The problem becomes so simplified and devoid of laborious uninstructive technique that it seems almost superficial. One may learn as much from the living yolk-sac in an hour of careful study as in almost a week's perusal of sections. Most important is the fact that certain things may actually be seen to occur that sections could scarcely stimulate the mind to imagine. The only disadvanage is that the worker may be led to wonder whether so apparently simple a problem is actually of scientific importance. Fortunately, this mental state is soon passed over on realizing the necessary care and precaution which must be taken in following the movements and changes in the living cells.

Each cell is to be recognized as a living complex and the observer will realize the importance as well as the difficulties of thoroughly understanding and interpreting correctly its manifold changes and behavior. Material which to some extent allows such a study is often available. The *Fundulus* yolk-sac, however, is exceptionally adapted to this study on account of the beautiful simplicity of its structure, as well as the remarkable clearness with which each cell may be observed.

The results of this investigation of wandering mesenchymal cells may be summarized as follows:

The wandering cells begin to migrate away from the embryonic shield or line of the embryonic body at an early period, when the embryo is about forty hours old, the germ ring having almost completely passed over the yolk sphere to enclose its vegetal pole. The cells migrate away chiefly from the caudal end of the embryo, only a few wandering out from the head region. The regions of the yolk-sac thus suggest an area opaca about the tail end and an area pellucida around the neighborhood of the head.

All of the cells wander into the so-called subgerminal cavity, the space Wilson[1] and others consider a late stage of the segmentation cavity, between the yolk-sac ectoderm and the periblast syncytium.

When the cells first appear they are all closely similar in shape and about the same size. Very soon, however, they begin to exhibit certain differences. Many become elongate spindle cells with delicate filamentous processes, sometimes producing a stellate appearance. Others are more ameboid in shape with conical pseudopod-like processes which are constantly being thrown out at one place and withdrawn at another. Still a third class of cells appears somewhat later than the other two; these are more circular in outline with short pseudopods and are more slowly moving.

The movements of these extremely numerous cells and their changes of position may be readily followed with a high magnification. In embryos of about sixty hours, still some time before the heart begins to beat or the blood to flow, four clearly distinct types of cells can be recognized among these originally similar mesenchymal cells, and the further history of the four types may be completely traced.

The ameboid cells with conical pseudopod-like processes shortly after sixty hours begin to show an accumulation of pigment granules within their cytoplasm. Just at this time they are seen to be of two distinct varieties,

[1] Wilson, H. V., "The Embryology of the Sea Bass (*Serranus atrarius*)," *Bull. U. S. Fish Com.*, 1891.

one depositing a black and the other a brownish-red pigment.

The black chromatophore increases rapidly in size and by the end of the third day becomes an enormous ameboid body wandering over the yolk. These cells are attracted to the walls of blood vessels and plasma-filled spaces, such as the pericardial cavity becomes in individuals without a blood circulation. When the embryo is five days old the chromatophores are abundantly arranged along the walls of the vitelline vessels, but the pigmented cells are distinctly separate. After this time neighboring cells begin to fuse along their adjacent borders and large pigment syncytia are formed which completely surround and ensheath the vessels. A single syncytium is often of considerable extent.

The brown chromatophores have a somewhat different history. They never become so massive as the black, and their processes are more delicate and graceful in appearance. Yet these cells also attain a large size and in embryos of 72 hours are scattered over the entire yolk-surface. After the third day when the blood begins to flow in the yolk vessels, the brown chromatophores likewise become attracted to the vessel wall. These exquisitely branched cells apply themselves to the wall of the vessel and may often completely surround it. This type of chromatophore, however, always maintains its cellular individuality and never fuses with other cells to form a syncytium, as is the case with the black type.

The function of the chromatophores on the yolk-sac is most difficult to decide, but one thing is certain, they never become changed into any type of blood cell. The brown chromatophore in early stages may accidentally reach the blood current; it then becomes spherical and may readily be observed for a long time on account of its huge size as compared with the blood cells. It never, however, changes in type.

In specimens without a circulation of the blood both types of chromatophores arise in a normal manner and differentiate normally. Their arrangement along the vessel walls fails to occur. The chromatophores, therefore, remain scattered over the yolk or collected about the plasma filled spaces. The heart in such embryos is sheathed with pigment, while the normal heart never has a chromatophore on it.

The elongate spindle cells with their delicate filamentous processes are small in comparison with the two chromatophore types. These spindle cells retain in general their original appearance, but their behavior is most important. In embryos of about forty-eight hours such cells aggregate into certain rather definite groups; later, the groups become more linear in shape and finally these lines of cells arrange themselves so as to form tubular vessels. Several of the larger vessels arise independently upon the yolk, and certain ones of them later become connected with the venous end of the heart, while in all cases capillary nets which also arise independently become connected with the larger vessels. These processes may actually be followed through every step in the living yolk-sac.

The wall of the early vessels is very irregular, with spaces existing between the component cells. Corpuscles are often caught in these spaces or are entangled in the filamentous processes of the endothelial cells. Such conditions in sections would appear as though the corpuscles actually formed a part of the endothelial wall and might incorrectly be interpreted as endothelial cells changing into blood cells. Nothing has been seen in the living embryos to indicate that an endothelial cell has the power to produce a blood cell or to change into a blood cell of any type, but much has been seen to the contrary.

The generalization strikingly made by Thoma[2] that larger vessels arise from a network of capillaries is not true for the large vitelline vessels of the fish yolk-sac. In the specimens without a circulation of the blood the vessels arise and increase in size and persist for a long time without ever experiencing any effect of the blood current upon their walls.

[2] Thoma, R., "Untersuchungen ueber die Histogenese und Histomechanik des Gefässsystems," Stuttgart, 1893, and "Text-Book of General Pathology and Pathological Anatomy," trans. by Bruce, London, 1896.

In many embryos the circulation after having begun may stop for a time and then later be reestablished, the vessels having persisted in a normal condition. Thoma's so-called laws of vessel formation are, therefore, rudely violated by the development of the vascular system in these embryos.

The vessels arising from independent mesenchymal cells in the space of the blastocele in the teleost yolk-sac entirely overthrow any notion that vessels arise ontogenetically as portions of the celomic epithelium. The vascular lumen is originally continuous with the primary body cavity, the segmentation cavity, and never with the secondary body cavity, or celomic cavity.

The fourth class of cells wander out from the embryonic body somewhat later than the three former types. These are small circular cells with short pseudopod-like processes. They move very slowly, but finally collect into groups on the posterior and ventral regions of the yolk-sphere.

The round cells wander away only from the caudal region of the embryo and probably are derived from the so-called intermediate cell mass which is the anlage of the red blood corpuscles in the fish embryo.

The groups of round cells are slow in their differentiation but just before the circulation of the blood begins, they are seen to be circular erythroblasts. The observer may follow the disappearance of the islands of cells one by one as they are enclosed by the vessels and swept into the circulating stream. About the fifth day these circular erythroblasts become flattened ellipsoidal erythrocytes filled with hemoglobin, the typical red blood corpuscle. The complete change from wandering, more or less globular mesenchymal cells into typical hemoglobin-bearing corpuscles may be followed in the living yolk-sac.

In several instances the body proper of the embryo failed to develop or else degenerated very early, yet the yolk-sac formed or persisted with numerous blood islands fully differentiated.

The embryos in which there has been no circulation of the blood form the blood islands from the wandering cells on the yolk-sac, and the constituent elements of these islands differentiate perfectly and may maintain their red color for many days. Yet they never leave the locality in which they have differentiated. The fully formed red blood corpuscles have little if any power of migrating. When the observer can be positive that the blood has never circulated, and this requires very consistent watching, the blood islands of the yolk-sac are always limited to certain regions, and never occur so far anteriorly on the ventral surface of the yolk as to reach the venous end of the heart.

Finally, we may consider the study of the developmental products of the early wandering mesenchymal cells on the yolk-sac of the *Fundulus* embryo as a problem of cell lineage followed to its ultimate end. The primordial mesoderm cell or cells carry within their bodies all the potentialities of the mesoderm and may give rise to a series of cells which are capable of developing muscle, cartilage, bone, connective tissue proper, blood cells, vessels, etc. Yet after a few cell generations the individuals in the series derived from these early cells containing all the mesodermal potentialities no doubt become somewhat limited as to their potentialities. In a certain generation there may be definite cells more or less generally distributed which possess the capacity to give rise to muscle cells, but to no other type of mesodermal tissues. Still later in development these cells may become even more limited in their developmental capacities and thus have the power to produce only a certain type of muscle cell and no other type.

Collections of such cells would then be designated embryologically as the anlage of striated muscle, smooth muscle or heart muscle, as the case might be. Yet it is not to be forgotten that at this stage there might be really no means of distinguishing between the several different types of mesodermal cells.

Limitization of potentialities in the individual mesenchymal cells has apparently reached a comparable stage just about the time when the cells begin to wander upon the yolk-sac of *Fundulus*. We have seen these cells as

they wander out and have noted how very soon they may be separated into four distinctly different types, and following the development and behavior of these types it has seemed evident that they are entirely separate and do not intergrade or transmutate. The black chromatophore does not change its nature or divide off other cells which become different in type from the parent cell. Neither do the endothelial cells lining the vessel walls change into chromatophores or into erythroblasts, or vice versa.

From the observations on these yolk-sacs we must conclude that the four types of cells described above have developed from four different anlagen, although these anlagen were not necessarily localized groups of cells, but were diffusely scattered mesenchymal cells capable of developing into a definite product, either normal or abnormal, depending upon the nature of the developmental environment. Therefore, the four distinct mesenchymal anlagen each gives rise to a perfectly typical and distinct cell type, although all develop in, as far as is possible to judge, an identical environment, the cavity of the yolk-sac between the ectoderm and the periblastic syncytium. The differences among the four cell types produced are from the standpoint of our present knowledge in all probability due to the potential differences among the apparently similar mesenchymal cells from which they arose. The four types including endothelial cells and erythrocytes we must consider, from an embryological standpoint, as being polyphyletic in origin.

C. R. STOCKARD

WOODS HOLE, MASS.,
September 15, 1915

ANTHROPOLOGY AT THE SAN FRANCISCO MEETING

A SPECIAL meeting of the American Anthropological Association was held in the Museum of Greek Sculpture and Anthropology, University of California, Berkeley, August 3 to 5, 1915, in affiliation with Section H and the American Anthropological Association. In the absence of Professor A. L. Kroeber, chairman of the committee on program, Professor T. T. Waterman, vice-chairman, presided. Although the program was a comparatively short one, the attendance at the meetings was large.

Papers of interest to anthropologists were also read before the joint meeting of the American Psychological Association and Section H; and before the Archeological Institute of America. However, the abstracts which follow will be confined entirely to the papers read before the Anthropological Association. For example, among the papers read before the Archeological Institute should be mentioned "Ancient Mexican Spindle-whorls," by Mrs. Nuttall, which was illustrated by an exhibit of two hundred specimens, as well as by reference to one of Lord Kingsborough's volumes; "Life Forms in the Pottery of the Southwest," by Mrs. Harry L. Wilson; "Aspects of Neolithic Culture of the Santa Barbara Channel Islands, California," by Hector Alliot; "Latest Work of the School of American Archeology at Quirigua, Guatemala"; and "Archeology at the Panama-California Exposition," by Edgar L. Hewett; and "The Unpublished Material in the Mayance and Southern Mexican Languages," by Wm. E. Gates.

The papers read before the American Anthropological Association included: "A Demonstration of the Skull of an Ancient San Diegan Indian Showing the Largest Coronoid Index yet Recorded" (by title), by J. C. Thompson; "Differences in Papago and Pima Coiled Basketry" (by title), by Mary Lois Kissell; "Kumana, a Primitive Corner of Japan, and Its Folk-Lore, as Studied by Mr. Minkata" (by title), by W. T. Swingle; and "The Significance of the Present Forward Movement in China," by Yamei Kin.

Abstracts of all the other papers presented follow:

The Miwok Moieties: E. W. GIFFORD.

The Central Sierra Miwok Indians of the Sierra Nevada Mountains of California are divided into exogamous moieties with paternal descent. Each moiety is associated through the personal names of its members with either the "water" or the "land" side of nature, this division of nature being more or less arbitrary. The object after which a person is named does not appear, as a rule, in the name itself; it does appear, however, in the connotation of the name. The connection thus existing between the moiety and a group of natural objects lends a totemic aspect to the Miwok moieties, which is supported by a myth attributing the parentage of the founders to the bear and the coyote. The moieties are practically impotent as

ceremonial factors, their chief function being the regulation of marriage.

The system of relationship contains thirty-four terms, and in certain features closely parallels the systems of the southern Siouan tribes, notably the Omaha. A striking feature is the placing of cross cousins in two generations, which results in the use for cross cousins of terms meaning son, daughter, stepmother, uncle, niece, nephew. This feature is correlated with the marriage of a woman to her father's sister's husband.

Cross cousin marriage is limited to one pair of cousins only, who use the terms meaning son and stepmother. The theory is advanced that the Miwok type of cross cousin marriage originated through the influence, upon the institution of marriage, of wife purchase and descent in the male line.

Demonstration of a Series of Philippine Skulls from Bohol: Luther Parker.

The number of skulls under discussion is seven. These skulls were collected in a limestone burial cave on a promonotory near the barrio of Tiagas in the town of Loay, Bohol Province, by the writer personally. In addition to the skulls, he also secured the lid of a coffin, some ornaments and pieces of pottery both native and Chinese. A few steel or iron spear heads and parts of blades were likewise obtained.

It is probable that the articles collected were deposited in the cave not later than A.D. 1600, since the conversion of Bohol to Christianity took place about that time. As to how much earlier the cave was used it is not possible to state, but probably not earlier than A.D. 500. Native traditions do not fix the time of these burials, but deal only with the custom of borrowing the pottery and ornaments for use in fiestas, this custom having been quite widely distributed.

Cave burial seems to have been practised quite extensively in the Bisayas and to some extent in northern Luzon among the Igorots and in the Batanes Islands. Jar burial both in caves and out was also practised in the Bisayas and in the Bobuyanes north of Luzon. Jar burial is practised among the interior tribes of north Borneo. Cave burial occurs in northeast Borneo. Jager wrote of cave burials in the Bisayas and H. Ling Roth discusses the subject quite thoroughly as relating to north Borneo.

The following table gives the measurements obtained by the writer. They are subject to correction by a more experienced observer:

Serial No.	Cephalic Ind.	Nasal Ind.	Orbital Ind.	Height	Capacity
1	77.0	53.5	88.6	138.5 mm.	1,400 cc.
2	75.2	50.0	90.0	140.5	1,420
3	93.7	Not obtainable	94.7	129.5	1,280
4	80.2	52.7	97.0	133.5	1,280
5	91.3	Not obtainable	86.4	127.5	Broken
6	93.7	57.1	84.2	120.0	Broken
7	100.	Not obtainable	87.2	132.0	1,160

As may be seen by the above tables, the skulls were not of a uniform type, but ranged from dolichocephalic to brachycephalic. This corresponds with results obtained by other observers in Malaysia and serves to confirm what is known from other sources, viz.: that the Filipinos, as all Malayans, are very much of a mixture approximating the Japanese, Mongolian and Negritos in certain measurements.

These skulls are quite similar to living types in the neighborhood of their collection. Several of the skulls have been artificially deformed evidently by means of the "tadal" which was in use among the Milanaus of the Bintula River until recently. Jager secured deformed skulls from caves near Samar and Dr. Virchow measured and discussed them. The area of deformation seems to have included Sumatra (Rejang district), north Borneo, Ulitea Island and a part of the Bisayan group, especially Bohol and the Leyte-Samar district.

Race in the Pacific Area with Special Reference to the Origin of the American Indians: 1. Antiquity of Occupation: George Grant MacCurdy.

The Pacific washes the shores of both the Old World and the New; hence the Pacific area is a large one. It is at least indirectly in connection with the birthplace of man, for it is accessible from all the great land masses. Whether the American or Asiatic portion of this area was first occupied by man is a question of wide interest. An answer to this question would be of help in locating the spot, if indeed it was a single one, from which man has spread over the face of the earth.

Physically man is a vertebrate and belongs to the great class of so-called Mammalia. We may differentiate still further and place man in one of the Families composing the order of Primates, which includes not only the Simiidæ, but also the lemurs. Eocene lemurs are found in both the western (Puerco beds of North America) and eastern hemispheres. The Simiidæ, however, the family most nearly approaching man in physical structure, all belong to the Old World; the gorilla and chim-

panzee, to Africa, and the orang and gibbon to the Far East. The presumption is strong, therefore, that the human race also originated in the Old World.

To the Pacific area belong the well-known fossil ape-man from Java, *Pithecanthropus erectus*, which according to the associated fauna and flora is of lower Pleistocene age.

The Selenka Trinil Expedition of 1907–08, one of whose results was to reduce the age of *Pithecanthropus* remains from Pliocene to lower Pleistocene, secured a tooth that is said by Dr. Walkoff to be definitely human. It is a third lower molar found not at the Trinil site, but in a neighboring stream bed and in deposits older (Pliocene) than those in which *Pithecanthropus* occurred. Should this prove to be the case, *Pithecanthropus* could no longer be regarded as a precursor of man; it would give us instead the cross section of a different limb of the Primate tree from the limb whose branches represent the various types of Hominidæ.

The principal event of the Australian meetings of the British Association for the Advancement of Science one year ago was the presentation of a fossilized human skull from Darling Downs on the border between New South Wales and Queensland. Unfortunately this specimen was not found *in situ;* but is in the same state of fossilization as are the remains of extinct animal species from the same locality. The latter are said to be of Pleistocene age. The "solidly fossilized" human skull (that of a youth) is evidently not of Neandertal type; nevertheless the authorities present were of the opinion that it represents an extremely primitive type. When archeologists become thoroughly awake to the possibilities of China a new chapter in the antiquity of occupation of the Pacific area will in all probability be recorded.

Passing to the American Pacific shores a good deal has already been accomplished especially in California; but the results do not point to a great antiquity of occupation. Man probably entered the Americas by way of Bering Strait after the final retreat of the last maximum glaciation. Bearing directly on this point is the discovery in 1912 by Dr. Hrdlička of vestiges of an ancient population in northeastern Asia persisting there perhaps since late Paleolithic times, and which possibly gave rise to the American Indian. This is in line with the results of the Jesup North Pacific Expedition, and future archeological discoveries may confidently be expected to support the same point of view. As a seat of human occupation, therefore, China probably antedates Mexico and Peru.

Antiquity of Man in California from the Point of View of the Paleontologist: JOHN C. MERRIAM.

In working over the collections of the state geological survey of California in 1894, the writer's attention was particularly attracted by a collection of mortars and other objects of human manufacture, which, according to the accompanying labels, had been found in the Pleistocene gold-bearing gravels of California. Accompanying these collections were other objects of a similar nature reported by reputable observers to have been obtained in formations not younger than Pleistocene. Although the objects in question were not unlike implements manufactured by the Indians of California within very recent time, the evidence favoring their antiquity appeared so remarkably definite that it seemed worth while attempting to secure all the facts bearing on the question of the occurrence and age in order to obtain some explanation of the evident inconsistency. Numerous inquiries among intelligent observers of good repute regarding the occurrences of human remains and relics in the auriferous gravels of California brought out a considerable amount of information as to the finds already known, and added several important occurrences to the list available. At this time there were known to the writer not less than eleven cases in which, from the point of view of the unprejudiced observer there seemed no question but that artificially fashioned objects had been found to be original constituents of Pleistocene or earlier formation of the California region.

The plan of work thus outlined was beginning to furnish small results when the organization of the department of anthropology at the University of California, through the generosity of Mrs. Phoebe A. Hearst, made it possible to carry out the whole scheme of investigation, only a small portion of which it had seemed possible for the writer to undertake individually up to that time.

While it is evident that the human race did not originate in America, its occupation of the western hemisphere has apparently covered a long period measured in years, if it does not actually extend back to an earlier geological period. At any rate, the advent of man far antedated the beginning of the American historic period and the approximate determination of the date of his arrival, whenever it may have occurred, furnishes an important field for investigation in American history.

As a field for investigation of geologically ancient types of the human race, North America has been notably barren. Even those who have been convinced that man was present on this continent before the beginning of the present geological period must concede that the evidences of his existence are much less common here than in most parts of the Old World.

Of the several widely known discoveries of human remains and relics reputed to represent a geologically ancient type of man on this continent, some of the occurrences reported from California have most persistently forced themselves on the attention of the investigator, though not always receiving general recognition as of scientific value.

It has been realized at the outset that any satisfactory conclusions in a work of this character are not to be arrived at within narrow time limits, and that no single mode of attack may be considered sufficient in itself. At the outset four lines of investigation were laid down: (1) Tracing man back from the known type to the unknown, through an investigation of the great shell mounds of the coast region, the most critical study being given to the lowest or earliest deposits. In this work we go from the known culture of the uppermost layers of the mounds back to a period in which conditions were quite different from those under which the recent Indians appear to have lived. (2) The thorough investigation of all cave deposits, whether recent or Quaternary, with particular reference to possible human occupation. (3) A careful study of these Quaternary or recent alluvial formations in which the occurrence of human remains or relics appears to be possible. This comprised a study of many Quaternary formations and the collection in them of all obtainable fossil remains. (4) A careful review of all the evidence relating to the reputed occurrence of implements or human remains in the Auriferous gravels, or other ancient deposits of a similar nature in California.

The result of investigations along the various lines followed in the original plan for the department of anthropology has shown that in a considerable number of cases fragmentary human remains or fragments of stone and bone worked by man have been found in association with Pleistocene deposits in California, but that in every case a very considerable doubt attaches to the occurrence, so that in no instance do we have in California an undoubted occurrence either of human bones or of implements made by man in such association with Pleistocene deposits as to prove the Pleistocene age of the human relics. While remains of man are known in many localities of undoubted Pleistocene age in the Old World, and while an age in years amounting to many tens of thousands and perhaps many hundreds of thousand years can be ascribed to these remains, we have yet to show in California the relics of man's occupation dating back to more than ten or twenty thousand years.

It is possible that man coming from the Old World, the place of origin of the human race, has at various times colonized the North American continent, but was unable to secure a permanent foot-hold, and because of the brief period of his occupancy has left no ancient relics. Human history may have waited until a comparatively recent time for the occupation of the western hemisphere by man in such force as to make his conquest of the region permanent.

Time Perspective in American Culture, a Study in Methods: Edward Sapir.

A historical science, such as cultural anthropology is, must have chronological perspective. The methods available for the determination of this perspective in aboriginal American culture are partly direct, partly inferential. The simplest type of direct evidence is that contained in the statements of early travelers and noted writers. A second type is embraced in the statements of the natives themselves. The third and most valuable type of direct chronological evidence is obtained by studying the stratigraphy of archeological remains.

The inferential evidence may be derived from the data of physical anthropology, ethnology or linguistics. Conclusions of historical value may be drawn from the persistence of a type in a certain area, and from the denseness of population. Ethnology yields a considerable number of methods for the inferring of time sequences. These may be classed into three groups as the seriation method (*e. g.*, inferences based on the relative degree of development of elements forming a natural sequence); the association method, which can be employed in a considerable number of ways (*e. g.*, by inferring chronological priority of one of two cultural elements because of its entering into a greater number of associations with other elements); and the distribution method. Linguistic evidence may be utilized for the chronology of culture partly by the study of native terms for various culture concepts, partly

by inferences based on the distribution of languages.

The ultimate task of constructing a general picture of the development of culture in America can not be undertaken without the cumulative evidence derived from all possible methods, direct and inferential, for constructing cultural time sequences.

The Zodiacal Basis of the Snake Dance and other Hopi Ceremonials: STANSBURY HAGAR.

The purpose of this paper is to present evidence which tends to show that the ritual of the twelve monthly festivals of the Pueblo Indians of Arizona and New Mexico is based upon zodiacal symbolism, in other words, that the features of each festival refer to the attributes of the native zodiacal signs through which the sun is passing at the time when the festival is held. This interpretation is supported: (1) by the rites directed each month towards one of the twelve white marks distributed around the circular sacred kiva or religious edifice at Zuni, as described by the late Frank Hamilton Cushing; (2) by the association of the sun and star gods as the two principal actors in the winter solstice ceremony at Walpi; (3) by the determination of the moment of beginning the principal ritual of the Manzrau and other festivals at Walpi by observation of the zodiacal constellation in opposition to the sun at the moment of midnight or approximately then; (4) by the repetition of each festival in a minor manner at a date six months distant from the principal performance, the minor festival being directed toward the zodiacal sign in opposition to the sun through the sign. But more impressive than this internal evidence from the writer's viewpoint is the fact that the features of all twelve of the Pueblo festivals *in sequence* correspond with the features of the festival celebrated at the same time amongst the ancient Mexicans and the Maya of Yucatan; and the writer has established at least to his own satisfaction, in papers published in the *American Anthropologist* and the publications of the International Congress of Americanists, that this Mexican and Maya ritual was based upon this same zodiacal symbolism. Such a sequence of symbolism can not be created by chance nor constituted by inauguration. It is revealed in star charts and constellation symbols in the codices, in mural paintings and in the design of sacred cities as well as in the ritual referred to; and if zodiacal in Mexico, the same sequence of symbolism can hardly be otherwise than zodiacal amongst the Pueblos.

Probably in no religion did astronomy play a more important part than in that of ancient America from Peru to Arizona, and this zodiacal symbolism gives us the key to its significance. But in all this vast area only amongst the Pueblos can the astronomic ritual still be seen.

The Snake Dance, to-day the most famous ritual of the American Indian, is but a subordinate episode of the festival which should be known as that of the mountain lion held when the sun is passing through the zodiacal constellation known to the Pueblos, Mexicans and Maya by the name of that animal. It is our sign Leo, the lion. And in the following month the women's Maize Festival of the Pueblos honors the Maize Goddess as in Mexico, Yucatan and Peru when the sun is passing through the sign of the Maize Mother. She is our Virgo, the celestial mother of cereals.

The Octopus Motive in Ancient Chiriquian Art: GEORGE GRANT MACCURDY.

In the ancient pottery of Chiriqui, one soon learns to associate a given motive with a given paste, slip, quality of modeling and the character and number of the colors employed as well as the method of their application. Thus we find the plastic armadillo dominating the great group which might appropriately bear that name; the incised serpent goes with a distinctive group of black ware; the plastic fish in the guise of tripod supports runs through another group; while the painted alligator is supreme in two closely related groups of painted ware. The most puzzling designs were on the so-called lost color ware. A key to their meaning recently came to light in the shape of a more realistic rendering of the motive than had been known hitherto. The design, called to my attention by Professor M. H. Saville, and recently published by me, represents an octopus.

A further study tends not only to confirm what was said in my last note, but also to emphasize the importance of this newly discovered motive as the one distinctive feature of lost color symbolism in ancient Chiriquian art.

On a reexamination of the lost color ware, the octopus design is found to appear unmistakably under one guise or another on perhaps nine tenths of all the lost color vases hitherto published; a cursory study of the large duplicate series in the Yale Museum shows that at least as large a percentage holds true of unpublished specimens. If a new name were needed for this large group, *Octopus* ware would thus be most appropriate.

Eugenics and its Natural Limitations in Man: Aleš Hrdlička (not present).

Human Eugenics may be defined as the "science of improving the human stock." It stands in many respects on quite a different footing from eugenics of organisms other than man. The term is relatively new; it relates to a seemingly new and most promising as well as timely scope of activities; and due to its appeals to popular imagination, and in common with other newly appearing branches of the science of man in the past, it has been and is now much sinned against. It has been permitted and even used to arouse hopes which at best can not be realized except at a very slow pace and in the course of great length of time.

To improve man it will self-evidently be necessary first to know thoroughly: (1) the stock to be worked upon; and (2) what constitutes improvements in the same; then the worker will be confronted with a most important problem, namely, how to effect the improvements, and how to make them permanent or even progressive, and thoroughly wholesome.

The knowledge of the stock implies perfect anatomical, anthropological, physiological, pathological and especially chemical understanding. How far we are in all these respects from the goal is well appreciated by the more advanced students in these different branches of learning.

As to what would constitute improvement in the human stock a general agreement will probably be reached on the following:

(1) Universal bettering of health;
(2) Fortification against infectious or contagious diseases or immunization;
(3) Elimination of hereditary defects and untoward predispositions;
(4) Increase in nervous power and resistance;
(5) Increase in sensorial efficiency;
(6) Progressive improvement in mentality; and
(7) A general, fixed harmony of all results, that would strengthen and not adversely interfere with the vital functions of the body.

Reflections on the above with our actual knowledge of humanity will readily show the many and great limitations that confront the "science of improving man." We know at best only superficially what we deal with even in the case of our own person; we never learn the whole inheritance of any man or woman; we deal, not with simple mathematical propositions, but with intricate combinations of qualities and quantities in each subject; in a great majority of cases we know not as yet how to remove or compensate for a given defect, or how to strengthen permanently and especially create a desirable quality, or how to prevent or cause the transmission of tendencies or qualities. And we have and shall probably continue to have only insignificant control of subjects in the vital matters of mating, living, environment.

Viewed thus very soberly, the new science in its application to man loses much of its fancied luster; if it succeeds in becoming established as a separate branch of learning, as it has in reference to lower forms, it will be welcomed as a helping sister, confronted with hard work, modest in the appreciation of the difficulties which lie before it and grateful for all past and future assistance. After novelty wears off, it will follow patiently in the slow laborious footpath started upon long ago by the physician and surgeon and then by the students of mankind in general. But it is still somewhat questionable if human eugenics really can prove itself to possess a sufficiency of distinctive attributes to proceed as a separate branch of science.

Migration and Culture: Robert H. Lowie.

Migration and culture are closely interwoven, indeed migration is often solely an inference from cultural facts, especially of a linguistic character. In point of clearly demonstrable migrations the two divisions of the Pacific area differ widely, those of Oceania being incomparably greater than those in western America. Accordingly there has also been a wider diffusion of cultural traits in Oceania. The important problem whether Polynesian seafarers ever reached America remains unsolved. Van Hornbostel has furnished good evidence from the point of view of theoretical music, but this evidence stands alone. There certainly has been no far-reaching influence of Oceania on New World culture.

Ethnologists are beginning to realize that the problem is not solved when similarities in culture are explained by transmission due to contacts. We must learn what particular features are adopted by the borrowing people; whether the borrowed elements are adopted mechanically or are assimilated to the preexisting culture of the people; and a host of other circumstances must be ascertained if our knowledge of cultural diffusion is to become more than superficial. The questions here indicated are among the most promising in the range of ethnology; and Dr. Rivers in England, and a number of American investigators, have made a fair beginning in attacking them.

George Grant MacCurdy,
Secretary